PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCII.

PART I.

LONDON,

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

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of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1801, the Medal on Sir GODFREY COPLEY's Donation, to Mr. ASTLEY COOPER, for his Papers On the Effects which take place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the removal of a particular species of Deafness.

ERRATA.

Page 133, line 2 and 3, *for* 38,3, *read* 383.
—— 134, — penult. *for* hyperoxygenized, *read* oxygenized.

PHILOSOPHICAL TRANSACTIONS.

I. *The Croonian Lecture. On the Power of the Eye to adjust itself to different Distances, when deprived of the Crystalline Lens. By* Everard Home, *Esq. F. R. S.*

Read November 5, 1801.

It is intended, on the present occasion, to state some facts and observations, in support of an opinion advanced in a former lecture, that the adjustment of the eye to see objects at different distances, does not depend upon any internal changes in the crystalline lens.

The first of the experiments which will be stated, was made with the assistance of the late Mr. Ramsden; and, had not the death of that valuable member of this Society deprived me of his further aid, the following observations would undoubtedly have been more deserving the attention of my learned audience.

It is impossible for me to mention Mr. Ramsden, from whom I have received so much assistance in every pursuit connected with optics and mathematics, in which I have been engaged,

without availing myself of this opportunity of paying that tribute of gratitude to his memory, which feelings of delicacy prevented me from offering to him while alive. It is unnecessary here to mention his genius, his merits, or his exertions for the promotion of science; these are equally well known to every member present, as to myself. It is only my individual obligations, in the prosecution of inquiries connected with the objects of this learned Society, that are meant to be taken notice of.

To his friendly and zealous assistance I am indebted for the information which was necessary to enable me to prosecute investigations upon the subject of vision; and, without such assistance, I should have shrunk from the inquiry. It is also to his early friendship, and his readiness to communicate to me his knowledge, that I look back, as among the sources of my early exertions, and love of philosophical pursuits.

In the year 1794, I laid before this learned Society some experiments, suggested and made by Mr. RAMSDEN, upon the comparative powers of adjustment of the eye, when in a perfect state, and when deprived of the crystalline lens. From the result of these experiments it appeared, that the removal of the lens did not deprive the eye of the power of seeing distinctly at different distances. As the person upon whom the experiments were tried did not see very distinctly, without a substitute for the lens, in making them, a double convex glass, of $2\frac{1}{4}$ inches focus, was placed before his eye; and, to render the image distinct, by correcting the spherical aberrations, the aperture was diminished to $\frac{3}{20}$ths of an inch; a less degree of diminution not answering that purpose.

The subject of these experiments was BENJAMIN CLERK, twenty-one years of age; one of his eyes was in a very perfect

state, and the other without defect, except what arose from the removal of the lens: and the results appeared to be satisfactory in deciding, that the eye, when deprived of the crystalline lens, retains a power of adjustment.

Opportunities of instituting experiments of this kind very rarely occur; the patients who have had their lenses extracted, either not seeing sufficiently well, or being too much advanced in life to be fit subjects for that purpose; but, in the year 1798, the following case came under my care, which enabled me to make some further observations, in confirmation of the former experiments.

HENRY MILES, a carpenter, at Westborough Green in Sussex, fifty years of age, applied, in the month of August, 1798, at St. George's Hospital, to be admitted as a patient, on account of blindness, from having a cataract in each eye; and was received under my care. Both the cataracts were extracted; and the eyes recovered from the effects of the operation, without suffering from inflammation. The right eye had the power of seeing objects with unusual distinctness; but the left was less perfect, the iris having been slightly torn, by the lens being too big to pass through the aperture, without injuring the membrane.

As soon as this man's eyes had recovered, I requested Mr. RAMSDEN to repeat some of the former experiments, on his right eye; which he readily agreed to do. Before the experiments were made, upon trying what was his power of vision with the naked eye, we were agreeably surprised to find that he saw so distinctly, as to admit of our ascertaining, without the aid of glasses, what were the ranges of his eye's adjustment.

A piece of pasteboard, with a letter of a moderate size, as an object upon it, was put into his hands; as he could not read, the

page of a book might have confused him: he was directed to vary the distance of the pasteboard from his eye, till he had ascertained the nearest and most distant situations, in which the object appeared distinct; these distances, by measurement, were 7 inches, and 18 inches. In repeating this experiment several different times, he brought the object very correctly to the same situations.

This result convinced Mr. RAMSDEN, that the eye possessed the power of varying its adjustment; and he did not think any more complex experiments would be nearly so satisfactory; consequently, no others were made, and the man was allowed to go into the country.

It was intended to make him a present of a pair of spectacles, allowing him to choose those best adapted to his eye; but his sight was so very good, that we entirely forgot it, till some time after he was gone.

These experiments confirmed the former ones so very strongly, and from their simplicity were so much less liable to error, that Mr. RAMSDEN and myself considered the object of our inquiry completely attained; the reason for not, at the time, laying them before this learned Society was, that they established no new fact, and the former ones did not appear to require their support.

This inquiry, always regarded as highly important by physiologists, has continued to engage their attention; and, in the BAKERIAN Lecture for last year, Dr. YOUNG has advanced some experiments to prove, that the adjustment of the eye to different distances, depends upon the crystalline lens: he considers the results of the experiments made by Mr. RAMSDEN, upon BENJAMIN CLERK'S eyes, as inconclusive; and the phenomena met

with, as arising from the smallness of the aperture, and not from any power of adjustment in the eye. Dr. YOUNG, therefore, with a view to obviate all possibility of deception in future, constructed an optometer, upon the principle of that of Dr. PORTERFIELD. In this instrument, when applied to presbyopic eyes, the eye, by looking along a line through a small convex lens, before which is placed a card with two narrow slits in it, near enough to each other to be within the limits of the pupil, will see the line as two lines, crossing each other at the point of perfect vision; and every eye that has the power of adjustment, will make the lines cross in different places, when adjusted to different distances.

With this instrument, Dr. YOUNG made experiments upon several eyes which had been deprived of the crystalline lens; and with all of them found, that the crossing of the lines was seen only at one point; he therefore concludes, that the power of adjustment was lost.

These experiments of Dr. YOUNG led me to reconsider the subject; and it was matter of regret that BENJAMIN CLERK was not in this country, as making a trial with the optometer on his eye, would have determined, in the most satisfactory manner, whether there had been a fallacy in the former experiments.

This not being in my power, I made inquiry after HENRY MILES, upon whom the second experiments were tried; and I had the pleasure to hear, that he was in good health, and that his eyes continued to have very distinct vision, so much so, that he never had occasion to make use of any glasses, from the time the operation had been performed.

With the view of making some experiments on this man's eyes, with Dr. YOUNG's optometer, I procured that instrument from

Mr. CARY, the optician, made exactly in the same manner as that which had been executed under Dr. YOUNG's direction. I first, however, tried the experiments upon my own eye; but had the mortification to find myself unable to make the lines cross in two different situations. This led me to try the eyes of several of my friends; who were equally unable to make the lines cross any where, except at one point. Young people, indeed all those under thirty years of age, were capable of varying the place of intersection; but none who were above forty, could produce any change in it.

As I could not doubt of my own eye having the power of varying its adjustment, I was led to believe that the instrument required some address in the management, which I had not acquired; and therefore despaired of making HENRY MILES sufficiently master of it, to do justice to my views.

To obviate these difficulties, I adapted the optometer, without the lens, to presbyopic eyes, by making a line 4 feet long, upon strong paper, divided into inches, and having the same slits to look through as in the other. This instrument, and Dr. YOUNG's, I put into the hands of my friend Sir HENRY ENGLEFIELD, with a request that he would examine them, and, when he had become perfectly master of them, and of the best mode of using them, that he would assist me in making experiments with them; for, as he was more in the habit of changing the focus of his eye, in using optical instruments, he would more readily detect the circumstance which prevented me from succeeding in the experiment.

After several trials with this optometer, and seeing its defects, Sir HENRY ENGLEFIELD improved it, by having the paper pasted upon a strong board, 4 feet long, which rendered the surface free from the slightest inequalities; and, instead of

a line marked with ink, a thread of black silk was stretched along the middle of the board. With this instrument, he found that his eye could make the lines cross at two different points, at several inches distance from each other. The readiest mode of making the experiment succeed, was first fixing his eye upon some near object, held above and a little on one side of the silk thread, and, when the focus of his eye was adapted to that distance, then to look at the thread; afterwards to look at some distant object, and, when that had become very distinct, again to look at the thread. Upon trying the instrument with my own eye, in this way, I found the crossing of the lines changed its situation, with every change of adjustment; and, after being accustomed to make this experiment, I was enabled to produce a similar change in the optometer with the lens, but by no means in so satisfactory a manner, nor did it last more than an instant; my eye probably not being so well fitted as many others, for experiments of this kind.

The optometer without the lens was hence admitted to be the most easily managed, by the eye of a person unaccustomed to such experiments, and therefore it was determined to make use of it in the trials upon HENRY MILES's eye; which we were enabled to do, as his vision was sufficiently distinct without the aid of glasses, and as, from never having used them, he saw much better with his naked eye.

The following experiments were made with the optometer without the lens, on the 27th of August, 1801.

The first trials were upon Sir HENRY ENGLEFIELD's eye; which, being most familiar with the use of the instrument, became a standard with which the others might be compared.

Sir HENRY ENGLEFIELD's eye made the lines to intersect

each other at 12¼ inches, as the near distance; and at 28½ inches, as the furthest distance. The experiment was repeated several different times, and the results were very nearly the same.

My own eye made the lines intersect at 12½ inches, as the near distance; and at 29½ inches, as the furthest distance.

A man servant of Sir HENRY ENGLEFIELD's, twenty-five years of age, made the lines intersect at 12 inches, and at 31½ inches.

HENRY MILES, fifty years of age, whose eye had been deprived of the crystalline lens for three years, made the lines intersect at 8 1/10 inches, as the near distance; and at 13 1/10, as the furthest distance.

This experiment was repeated two different times in the forenoon, with the same result, and again in the afternoon, without there being any considerable variation; but, upon trying it again, after the eye had been fatigued, he was unable to make the lines cross nearer than 11⅛ inches, although he could make them cross at 13 1/10 inches; so that adjusting the eye to a near distance, was more difficult after it had been much used, than before.

HENRY MILES was unable, in the optometer with the lens, to produce any change in the crossing of the lines, nor did he see them cross with sufficient distinctness to make us consider it a fair experiment.

The following experiment was made upon MILES's eye, at the suggestion of Sir HENRY ENGLEFIELD, with a view to ascertain in another though less decisive way, whether any change took place in it, when directed from a near object to a more distant one.

A piece of pasteboard, in which a black circle, about ¼ of an

inch in diameter, with a dot in the centre, had been described near to its edge, was placed perpendicularly to the horizon, at 5 inches distance from the eye; another piece of pasteboard, with a circle and dot in it, was placed at the distance of 18 inches; the farthest circle was made a little larger than the other, that it might appear equally distinct at the greater distance. When the eye was directed towards these two objects, they appeared upon the same level; and the circumference of the circles, had they been projected on the same perpendicular plane, would have been nearly in contact.

MILES was placed opposite these objects, with his head made steady, and prevented from moving: he was then told to look at one, till it became very distinct; and, when he had done so, this was removed, and he was directed to look at the other, which did not immediately appear to him with the same distinctness. This was equally the case, whether he looked from the near one to the distant one, or the reverse: the eye did not see the object to which it was so suddenly directed, with the same defined outline as that from which it had been withdrawn.

This man sees best in a strong light; and it was in that light all the experiments were made: he can see very well in any degree of daylight; but his eyes are much fatigued by candle-light. Upon examining the eye attentively, the pupil was rather larger than in perfect eyes; the iris was in a very perfect state; and the cicatrix of the wound, in the inferior part of the cornea, was scarcely visible.

The sight being so good, without the aid of glasses, is not common; and, had not the lenses been extracted in a public

hospital, before a number of spectators, some doubts might be entertained whether they had been removed.

From the experiments which have been stated, it appeared to Sir HENRY ENGLEFIELD, that MILES's eye was not deprived of its power of adjustment; and, by whatever circumstances my own judgment might be deceived, or rendered partial, there was nothing by which his could be biassed, as he could have no object in view, but the promotion of science. His knowledge of optics, and his habit of making experiments, are the best pledges of these having been as accurately performed as the nature of the subject admits of; for, certainly, the sources of fallacy, in optical experiments, are numerous. Those that have been related, to be made with perfect accuracy, should be tried upon the eye of a person skilled in optics, and accustomed to such experiments; and whose eye had been deprived of the crystalline lens, without having received the slightest degree of injury in any of its other parts.

The experiments were instituted in the Isle of Wight, which prevented me from requesting several of my friends to be present at them, whose knowledge of the subject would have made me desirous of their assistance.

HALLER mentions the case of a nobleman, from whose eye the crystalline lens had been extracted, who used glasses, and could see with them objects at different distances. As this was an observation made upon a particular friend of his own, and as he refers to PEMBERTON, who mentions a case of depressed crystalline lens, in which no such effect took place, it is natural to suppose, that he had given considerable attention to the subject; and that, although the experiments he instituted are

not mentioned, the opinion was not advanced, without what appeared to him sufficient authority.*

* Et lente ob cataractam extracta vel deposita, oculum tamen ad varias distantias videre, ut coram in nobili viro video, absque ullo experimento, quo eam facultatem recuperaverit. Et si enim tunc, ob diminutas vires, quæ radios uniunt, æger lente vitrea opus habet, eadem lens in omnia distantia sufficit.

HALLER. *Elementa Physiologiæ.* Tom. V. Lib. xvi. §. 25. p. 514.

II. *The Bakerian Lecture. On the Theory of Light and Colours. By* Thomas Young, *M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution.*

Read November 12, 1801.

ALTHOUGH the invention of plausible hypotheses, independent of any connection with experimental observations, can be of very little use in the promotion of natural knowledge; yet the discovery of simple and uniform principles, by which a great number of apparently heterogeneous phenomena are reduced to coherent and universal laws, must ever be allowed to be of considerable importance towards the improvement of the human intellect.

The object of the present dissertation is not so much to propose any opinions which are absolutely new, as to refer some theories, which have been already advanced, to their original inventors, to support them by additional evidence, and to apply them to a great number of diversified facts, which have hitherto been buried in obscurity. Nor is it absolutely necessary in this instance to produce a single new experiment; for of experiments there is already an ample store, which are so much the more unexceptionable, as they must have been conducted without the least partiality for the system by which they will be explained; yet some facts, hitherto unobserved, will be brought forwards, in order to show the perfect agreement of that system with the multifarious phenomena of nature.

The optical observations of NEWTON are yet unrivalled; and, excepting some casual inaccuracies, they only rise in our estimation, as we compare them with later attempts to improve on them. A further consideration of the colours of thin plates, as they are described in the second book of NEWTON's optics, has converted that prepossession which I before entertained for the undulatory system of light, into a very strong conviction of its truth and sufficiency; a conviction which has been since most strikingly confirmed, by an analysis of the colours of striated substances. The phenomena of thin plates are indeed so singular, that their general complexion is not without great difficulty reconcileable to any theory, however complicated, that has hitherto been applied to them; and some of the principal circumstances have never been explained by the most gratuitous assumptions; but it will appear, that the minutest particulars of these phenomena, are not only perfectly consistent with the theory which will now be detailed, but that they are all the necessary consequences of that theory, without any auxiliary suppositions; and this by inferences so simple, that they become particular corollaries, which scarcely require a distinct enumeration.

A more extensive examination of NEWTON's various writings has shown me, that he was in reality the first that suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works; and this by no less a mathematician than LEONARD EULER, whose system of light, as far as it is worthy of notice, either was, or might have been,

wholly borrowed from NEWTON, HOOKE, HUYGENS, and MALEBRANCHE.

Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the NEWTONIAN approbation, will probably be disposed to bestow on these considerations so much the more of their attention, as they appear to coincide more nearly with NEWTON's own opinions. For this reason, after having briefly stated each particular position of my theory, I shall collect, from NEWTON's various writings, such passages as seem to be the most favourable to its admission; and, although I shall quote some papers which may be thought to have been partly retracted at the publication of the optics, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment.

HYPOTHESIS I.

A luminiferous Ether pervades the Universe, rare and elastic in a high degree.

Passages from NEWTON.

"The hypothesis certainly has a much greater affinity with "his own," that is, Dr. HOOKE's, "hypothesis, than he seems "to be aware of; the vibrations of the ether being as useful and "necessary in this, as in his." (Phil. Trans. Vol. VII. p. 5087. Abr. Vol. I. p. 145. Nov. 1672.)

"To proceed to the hypothesis: first, it is to be supposed "therein, that there is an ethereal medium, much of the same "constitution with air, but far rarer, subtler, and more strongly "elastic.—It is not to be supposed, that this medium is one "uniform matter, but compounded, partly of the main phleg-"matic body of ether, partly of other various ethereal spirits,

" much after the manner that air is compounded of the phleg-
" matic body of air, intermixed with various vapours and
" exhalations: for the electric and magnetic effluvia, and gravi-
" tating principle, seem to argue such variety." (BIRCH. Hist. of R. S. Vol. III. p. 249. Dec. 1675.)

" Is not the heat (of the warm room) conveyed through the
" vacuum by the vibrations of a much subtiler medium than air?
" —And is not this medium the same with that medium by which
" light is refracted and reflected, and by whose vibrations light
" communicates heat to bodies, and is put into fits of easy re-
" flection, and easy transmission? And do not the vibrations of
" this medium in hot bodies, contribute to the intenseness and
" duration of their heat? And do not hot bodies communicate
" their heat to contiguous cold ones, by the vibrations of this me-
" dium propagated from them into the cold ones? And is not this
" medium exceedingly more rare and subtile than the air, and
" exceedingly more elastic and active? And doth it not readily
" pervade all bodies? And is it not, by its elastic force, expanded
" through all the heavens?—May not planets and comets, and
" all gross bodies, perform their motions in this ethereal me-
" dium?—And may not its resistance be so small, as to be
" inconsiderable? For instance, if this ether (for so I will call
" it) should be supposed 700,000 times more elastic than our
" air, and above 700,000 times more rare, its resistance would
" be about 600,000000 times less than that of water. And
" so small a resistance would scarce make any sensible altera-
" tion in the motions of the planets, in ten thousand years.
" If any one would ask how a medium can be so rare, let him
" tell me—how an electric body can by friction emit an exha-
" lation so rare and subtile, and yet so potent?—And how the

" effluvia of a magnet can pass through a plate of glass, with-
" out resistance, and yet turn a magnetic needle beyond the
" glass?" (Optics, Qu. 18, 22.)

HYPOTHESIS II.

Undulations are excited in this Ether whenever a Body becomes luminous.

Scholium. I use the word undulation, in preference to vibration, because vibration is generally understood as implying a motion which is continued alternately backwards and forwards, by a combination of the momentum of the body with an accelerating force, and which is naturally more or less permanent; but an undulation is supposed to consist in a vibratory motion, transmitted successively through different parts of a medium, without any tendency in each particle to continue its motion, except in consequence of the transmission of succeeding undulations, from a distinct vibrating body; as, in the air, the vibrations of a chord produce the undulations constituting sound.

Passages from NEWTON.

" Were I to assume an hypothesis, it should be this, if pro-
" pounded more generally, so as not to determine what light is,
" further than that it is something or other capable of exciting
" vibrations in the ether; for thus it will become so general and
" comprehensive of other hypotheses, as to leave little room for
" new ones to be invented." (BIRCH. Vol. III. p. 249. Dec. 1675.)

" In the second place, it is to be supposed, that the ether is a
" vibrating medium like air, only the vibrations far more swift
" and minute; those of air, made by a man's ordinary voice,
" succeeding one another at more than half a foot, or a foot

" distance; but those of ether at a less distance than the hun-
" dred thousandth part of an inch. And, as in air the vibra-
" tions are some larger than others, but yet all equally swift,
" (for in a ring of bells the sound of every tone is heard at two
" or three miles distance, in the same order that the bells are
" struck,) so, I suppose, the ethereal vibrations differ in big-
" ness, but not in swiftness. Now, these vibrations, beside their
" use in reflection and refraction, may be supposed the chief
" means by which the parts of fermenting or putrifying sub-
" stances, fluid liquors, or melted, burning, or other hot bodies,
" continue in motion." (BIRCH Vol. III. p. 251. Dec. 1675.)

" When a ray of light falls upon the surface of any pellucid
" body, and is there refracted or reflected, may not waves of
" vibrations, or tremors, be thereby excited in the refracting or
" reflecting medium?—And are not these vibrations propagated
" from the point of incidence to great distances? And do they
" not overtake the rays of light, and by overtaking them suc-
" cessively, do not they put them into the fits of easy reflection
" and easy transmission described above?" (Optics. Qu. 17.)

" Light is in fits of easy reflection and easy transmission,
" before its incidence on transparent bodies. And probably it is
" put into such fits at its first emission from luminous bodies,
" and continues in them during all its progress." (Optics. Second Book. Part III. Prop. 13.)

HYPOTHESIS III.

The Sensation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.

Passages from NEWTON.

" The objector's hypothesis, as to the fundamental part of it, " is not against me. That fundamental supposition is, that the " parts of bodies, when briskly agitated, do excite vibrations in " the ether, which are propagated every way from those bodies " in straight lines, and cause a sensation of light by beating " and dashing against the bottom of the eye, something after " the manner that vibrations in the air cause a sensation of " sound by beating against the organs of hearing. Now, the " most free and natural application of this hypothesis to the " solution of phenomena, I take to be this: that the agitated " parts of bodies, according to their several sizes, figures, and " motions, do excite vibrations in the ether of various depths " or bignesses, which, being promiscuously propagated through " that medium to our eyes, effect in us a sensation of light of a " white colour; but if by any means those of unequal bignesses " be separated from one another, the largest beget a sensation " of a red colour, the least or shortest of a deep violet, and " the intermediate ones of intermediate colours; much after " the manner that bodies, according to their several sizes, " shapes, and motions, excite vibrations in the air of various " bignesses, which, according to those bignesses, make several " tones in sound: that the largest vibrations are best able to " overcome the resistance of a refracting superficies, and so " break through it with least refraction; whence the vibrations

" of several bignesses, that is, the rays of several colours, which " are blended together in light, must be parted from one an- " other by refraction, and so cause the phenomena of prisms, " and other refracting substances; and that it depends on the " thickness of a thin transparent plate or bubble, whether a " vibration shall be reflected at its further superficies, or trans- " mitted; so that, according to the number of vibrations, inter- " ceding the two superficies, they may be reflected or transmitted " for many successive thicknesses. And, since the vibrations " which make blue and violet, are supposed shorter than those " which make red and yellow, they must be reflected at a less " thickness of the plate: which is sufficient to explicate all the " ordinary phenomena of those plates or bubbles, and also of " all natural bodies, whose parts are like so many fragments of " such plates. These seem to be the most plain, genuine, and " necessary conditions of this hypothesis. And they agree so " justly with my theory, that if the animadversor think fit to " apply them, he need not, on that account, apprehend a divorce " from it. But yet, how he will defend it from other difficulties, " I know not." (Phil. Trans. Vol. VII. p. 5088. Abr. Vol. I. p. 145. Nov. 1672.)

" To explain colours, I suppose, that as bodies of various " sizes, densities, or sensations, do by percussion or other " action excite sounds of various tones, and consequently vi- " brations in the air of different bigness; so the rays of light, " by impinging on the stiff refracting superficies, excite vibra- " tions in the ether,—of various bigness; the biggest, strongest, " or most potent rays, the largest vibrations; and others shorter, " according to their bigness, strength, or power: and therefore " the ends of the capillamenta of the optic nerve, which pave

"or face the retina, being such refracting superficies, when the "rays impinge upon them, they must there excite these vibra- "tions, which vibrations (like those of sound in a trunk or "trumpet) will run along the aqueous pores or crystalline pith "of the capillamenta, through the optic nerves, into the senso- "rium;—and there, I suppose, affect the sense with various "colours, according to their bigness and mixture; the biggest "with the strongest colours, reds and yellows; the least with "the weakest, blues and violets; the middle with green; and a "confusion of all with white, much after the manner that, in "the sense of hearing, nature makes use of aerial vibrations of "several bignesses, to generate sounds of divers tones; for the "analogy of nature is to be observed." (BIRCH Vol. III. p. 262. Dec. 1675.)

"Considering the lastingness of the motions excited in the "bottom of the eye by light, are they not of a vibrating nature? "—Do not the most refrangible rays excite the shortest vibra- "tions,—the least refrangible the largest? May not the harmony "and discord of colours arise from the proportions of the vibra- "tions propagated through the fibres of the optic nerve into "the brain, as the harmony and discord of sounds arise from "the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

Scholium. Since, for the reason here assigned by NEWTON, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it

becomes necessary to suppose the number limited, for instance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion less or more forcibly, by undulations differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species: and each sensitive filament of the nerve may consist of three portions, one for each principal colour. Allowing this statement, it appears that any attempt to produce a musical effect from colours, must be unsuccessful, or at least that nothing more than a very simple melody could be imitated by them; for the period, which in fact constitutes the harmony of any concord, being a multiple of the periods of the single undulations, would in this case be wholly without the limits of sympathy of the retina, and would lose its effect; in the same manner as the harmony of a third or a fourth is destroyed, by depressing it to the lowest notes of the audible scale. In hearing, there seems to be no permanent vibration of any part of the organ.

HYPOTHESIS IV.

All material Bodies have an Attraction for the ethereal Medium, by means of which it is accumulated within their Substance, and for a small Distance around them, in a State of greater Density, but not of greater Elasticity.

It has been shewn, that the three former hypotheses, which may be called essential, are literally parts of the more complicated NEWTONIAN system. This fourth hypothesis differs perhaps

in some degree from any that have been proposed by former authors, and is diametrically opposite to that of NEWTON; but, both being in themselves equally probable, the opposition is merely accidental; and it is only to be inquired which is the best capable of explaining the phenomena. Other suppositions might perhaps be substituted for this, and therefore I do not consider it as fundamental, yet it appears to be the simplest and best of any that have occurred to me.

PROPOSITION I.

All Impulses are propagated in a homogeneous elastic Medium with an equable Velocity.

Every experiment relative to sound coincides with the observation already quoted from NEWTON, that all undulations are propagated through the air with equal velocity; and this is further confirmed by calculations. (LAGRANGE. Misc. Taur. Vol. I. p. 91. Also, much more concisely, in my Syllabus of a course of Lectures on Natural and Experimental Philosophy, about to be published. Article 289.) If the impulse be so great as materially to disturb the density of the medium, it will be no longer homogeneous; but, as far as concerns our senses, the quantity of motion may be considered as infinitely small. It is surprising that EULER, although aware of the matter of fact, should still have maintained, that the more frequent undulations are more rapidly propagated. (Theor. mus. and Conject. phys.) It is possible, that the actual velocity of the particles of the luminiferous ether may bear a much less proportion to the velocity of the undulations than in sound; for light may be excited by the motion of a body moving at the rate of only one mile in the time that light moves a hundred millions.

Scholium 1. It has been demonstrated, that in different mediums the velocity varies in the subduplicate ratio of the force directly, and of the density inversely. (Misc. Taur. Vol. I. p. 91. YOUNG's Syllabus. Art. 294.)

Scholium 2. It is obvious, from the phenomena of elastic bodies and of sounds, that the undulations may cross each other without interruption. But there is no necessity that the various colours of white light should intermix their undulations; for, supposing the vibrations of the retina to continue but a five hundredth of a second after their excitement, a million undulations of each of a million colours may arrive in distinct succession within this interval of time, and produce the same sensible effect, as if all the colours arrived precisely at the same instant.

PROPOSITION II.

An Undulation conceived to originate from the Vibration of a single Particle, must expand through a homogeneous Medium in a spherical Form, but with different quantities of Motion in different Parts.

For, since every impulse, considered as positive or negative, is propagated with a constant velocity, each part of the undulation must in equal times have past through equal distances from the vibrating point. And, supposing the vibrating particle, in the course of its motion, to proceed forwards to a small distance in a given direction, the principal strength of the undulation will naturally be straight before it; behind it, the motion will be equal, in a contrary direction; and, at right angles to the line of vibration, the undulation will be evanescent.

Now, in order that such an undulation may continue its progress to any considerable distance, there must be in each part of it, a tendency to preserve its own motion in a right line from

the centre; for, if the excess of force at any part were communicated to the neighbouring particles, there can be no reason why it should not very soon be equalised throughout, or, in other words, become wholly extinct, since the motions in contrary directions would naturally destroy each other. The origin of sound from the vibration of a chord is evidently of this nature; on the contrary, in a circular wave of water, every part is at the same instant either elevated or depressed. It may be difficult to show mathematically, the mode in which this inequality of force is preserved; but the inference from the matter of fact, appears to be unavoidable; and, while the science of hydrodynamics is so imperfect that we cannot even solve the simple problem of the time required to empty a vessel by a given aperture, it cannot be expected that we should be able to account perfectly for so complicated a series of phenomena, as those of elastic fluids. The theory of HUYGENS indeed explains the circumstance in a manner tolerably satisfactory: he supposes every particle of the medium to propagate a distinct undulation in all directions; and that the general effect is only perceptible where a portion of each undulation conspires in direction at the same instant; and it is easy to show that such a general undulation would in all cases proceed rectilinearly, with proportionate force; but, upon this supposition, it seems to follow, that a greater quantity of force must be lost by the divergence of the partial undulations, than appears to be consistent with the propagation of the effect to any considerable distance. Yet it is obvious, that some such limitation of the motion must naturally be expected to take place; for, if the intensity of the motion of any particular part, instead of continuing to be propagated straight forwards, were supposed to affect the intensity of a neighbouring part of the undulation, an

impulse must then have travelled from an internal to an external circle in an oblique direction, in the same time as in the direction of the radius, and consequently with a greater velocity; against the first proposition. In the case of water, the velocity is by no means so rigidly limited as in that of an elastic medium. Yet it is not necessary to suppose, nor is it indeed probable, that there is absolutely not the least lateral communication of the force of the undulation, but that, in highly elastic mediums, this communication is almost insensible. In the air, if a chord be perfectly insulated, so as to propagate exactly such vibrations as have been described, they will in fact be much less forcible than if the chord be placed in the neighbourhood of a sounding board, and probably in some measure because of this lateral communication of motions of an opposite tendency. And the different intensity of different parts of the same circular undulation may be observed, by holding a common tuning fork at arm's length, while sounding, and turning it, from a plane directed to the ear, into a position perpendicular to that plane.

PROPOSITION III.

A Portion of a spherical Undulation, admitted through an Aperture into a quiescent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.

At the instant of admission, the circumference of each of the undulations may be supposed to generate a partial undulation, filling up the nascent angle between the radii and the surface terminating the medium; but no sensible addition will be made

to its strength by a divergence of motion from any other parts of the undulation, for want of a coincidence in time, as has already been explained with respect to the various force of a spherical undulation. If indeed the aperture bear but a small proportion to the breadth of an undulation, the newly generated undulation may nearly absorb the whole force of the portion admitted; and this is the case considered by NEWTON in the Principia. But no experiment can be made under these circumstances with light, on account of the minuteness of its undulations, and the interference of inflection; and yet some faint radiations do actually diverge beyond any probable limits of inflection, rendering the margin of the aperture distinctly visible in all directions; these are attributed by NEWTON to some unknown cause, distinct from inflection; (Optics, Third Book, Obs. 5.) and they fully answer the description of this proposition.

Let the concentric lines in Fig. 1. (Plate I.) represent the contemporaneous situation of similar parts of a number of successive undulations diverging from the point A; they will also represent the successive situations of each individual undulation: let the force of each undulation be represented by the breadth of the line, and let the cone of light ABC be admitted through the aperture BC; then the principal undulations will proceed in a rectilinear direction towards GH, and the faint radiations on each side will diverge from B and C as centres, without receiving any additional force from any intermediate point D of the undulation, on account of the inequality of the lines DE and DF. But, if we allow some little lateral divergence from the extremities of the undulations, it must diminish their force, without adding materially to that of the dissipated light; and their

termination, instead of the right line BG, will assume the form CH; since the loss of force must be more considerable near to C than at greater distances. This line corresponds with the boundary of the shadow in NEWTON's first observation, Fig. 1; and it is much more probable that such a dissipation of light was the cause of the increase of the shadow in that observation, than that it was owing to the action of the inflecting atmosphere, which must have extended a thirtieth of an inch each way in order to produce it; especially when it is considered that the shadow was not diminished by surrounding the hair with a denser medium than air, which must in all probability have weakened and contracted its inflecting atmosphere. In other circumstances, the lateral divergence might appear to increase, instead of diminishing, the breadth of the beam.

As the subject of this proposition has always been esteemed the most difficult part of the undulatory system, it will be proper to examine here the objections which NEWTON has grounded upon it.

"To me, the fundamental supposition itself seems impossible; "namely, that the waves or vibrations of any fluid can, like the "rays of light, be propagated in straight lines, without a con- "tinual and very extravagant spreading and bending every "way into the quiescent medium, where they are terminated "by it. I mistake, if there be not both experiment and demon- "stration to the contrary." (Phil. Trans. VII. 5089, Abr. I. 146. Nov. 1672.)

"Motus omnis per fluidum propagatus divergit a recto tra- "mite in spatia immota."

"Quoniam medium ibi," in the middle of an undulation

admitted, "densius est, quam in spatiis hinc inde, dilatabit sese "tam versus spatia utrinque sita, quam versus pulsuum rariora "intervalla; eoque pacto—pulsus eadem *fere* celeritate sese in "medii partes quiescentes hinc inde relaxare debent;—ideoque "spatium totum occupabunt.—Hoc experimur in sonis." (Princip. Lib. II. Prop. 42.

"Are not all hypotheses erroneous, in which light is supposed "to consist in pression or motion, propagated through a fluid "medium?—If it consisted in pression or motion, propagated "either in an instant, or in time, it would bend into the shadow. "For pression or motion cannot be propagated in a fluid in "right lines beyond an obstacle which stops part of the motion, "but will bend and spread every way into the quiescent medium "which lies beyond the obstacle.—The waves on the surface of "stagnating water, passing by the sides of a broad obstacle "which stops part of them, bend afterwards, and dilate themselves gradually into the quiet water behind the obstacle. "The waves, pulses, or vibrations of the air, wherein sounds "consist, bend manifestly, though not so much as the waves "of water. For a bell or a cannon may be heard beyond a "hill, which intercepts the sight of the sounding body; and "sounds are propagated as readily through crooked pipes as "straight ones. But light is never known to follow crooked "passages, nor to bend into the shadow. For the fixed stars, "by the interposition of any of the planets, cease to be seen. "And so do the parts of the sun, by the interposition of the "moon, Mercury, or Venus. The rays which pass very near "to the edges of any body, are bent a little by the action of the "body;—but this bending is not towards but from the shadow,

"and is performed only in the passage of the ray by the body, "and at a very small distance from it. So soon as the ray is "past the body, it goes right on." (Optics, Qu. 28.)

Now the proposition quoted from the Principia does not directly contradict this proposition; for it does not assert that such a motion must diverge equally in all directions; neither can it with truth be maintained, that the parts of an elastic medium communicating any motion, must propagate that motion equally in all directions. (Phil. Trans. for 1800. p. 109—112.) All that can be inferred by reasoning is, that the marginal parts of the undulation must be somewhat weakened, and that there must be a faint divergence in every direction; but whether either of these effects might be of sufficient magnitude to be sensible, could not have been inferred from argument, if the affirmative had not been rendered probable by experiment.

As to the analogy with other fluids, the most natural inference from it is this: "The waves of the air, wherein sounds consist, "bend manifestly, though not so much as the waves of water;" water being an inelastic, and air a moderately elastic medium; but ether being most highly elastic, its waves bend very far less than those of the air, and therefore almost imperceptibly. Sounds are propagated through crooked passages, because their sides are capable of reflecting sound, just as light would be propagated through a bent tube, if perfectly polished within.

The light of a star is by far too weak to produce, by its faint divergence, any visible illumination of the margin of a planet eclipsing it; and the interception of the sun's light by the moon, is as foreign to the question, as the statement of inflection is inaccurate.

To the argument adduced by Huygens, in favour of the

rectilinear propagation of undulations, NEWTON has made no reply; perhaps because of his own misconception of the nature of the motions of elastic mediums, as dependent on a peculiar law of vibration, which has been corrected by later mathematicians. (Phil. Trans. for 1800, p. 116.) On the whole, it is presumed, that this proposition may be safely admitted, as perfectly consistent with analogy and with experiment.

PROPOSITION IV.

When an Undulation arrives at a Surface which is the Limit of Mediums of different Densities, a partial Reflection takes place, proportionate in Force to the Difference of the Densities.

This may be illustrated, if not demonstrated, by the analogy of elastic bodies of different sizes. "If a smaller elastic body "strikes against a larger one, it is well known that the smaller "is reflected more or less powerfully, according to the diffe-"rence of their magnitudes: thus, there is always a reflection "when the rays of light pass from a rarer to a denser stratum "of ether; and frequently an echo when a sound strikes "against a cloud. A greater body striking a smaller one, pro-"pels it, without losing all its motion: thus, the particles of a "denser stratum of ether, do not impart the whole of their "motion to a rarer, but, in their effort to proceed, they are "recalled by the attraction of the refracting substance with "equal force; and thus a reflection is always secondarily pro-"duced, when the rays of light pass from a denser to a rarer "stratum." (Phil. Trans. for 1800. p. 127.) But it is not absolutely necessary to suppose an attraction in the latter case, since the effort to proceed would be propagated backwards without it, and the undulation would be reversed, a rarefaction

returning in place of a condensation; and this will perhaps be found most consistent with the phenomena.

PROPOSITION V.

When an Undulation is transmitted through a Surface terminating different Mediums, it proceeds in such a Direction, that the Sines of the Angles of Incidence and Refraction are in the constant Ratio of the Velocity of Propagation in the two Mediums.

(BARROW, Lect. Opt. II. p. 4. HUYGENS, *de la Lum.* cap. 3. EULER, *Conj. Phys.* Phil. Trans. for 1800, p. 128. YOUNG's Syllabus. Art. 382.)

Corollary 1. The same demonstrations prove the equality of the angles of reflection and incidence.

Corollary 2. It appears from experiments on the refraction of condensed air, that the ratio of the difference of the sines varies simply as the density. Hence it follows, by Schol. I. Prop. I. that the excess of the density of the ethereal medium is in the duplicate ratio of the density of the air; each particle cooperating with its neighbours in attracting a greater portion of it.

PROPOSITION VI.

When an Undulation falls on the Surface of a rarer Medium, so obliquely that it cannot be regularly refracted, it is totally reflected, at an Angle equal to that of its Incidence.

(Phil. Trans. for 1800, p. 128.)

Corollary. This phenomenon tends to prove the gradual increase and diminution of density at the surface terminating two mediums, as supposed in HYPOTHESIS IV; although HUYGENS has attempted to explain it somewhat differently.

PROPOSITION VII.

If equidistant Undulations be supposed to pass through a Medium, of which the Parts are susceptible of permanent Vibrations somewhat slower than the Undulations, their Velocity will be somewhat lessened by this vibratory Tendency; and, in the same Medium, the more, as the Undulations are more frequent.

For, as often as the state of the undulation requires a change in the actual motion of the particle which transmits it, that change will be retarded by the propensity of the particle to continue its motion somewhat longer: and this retardation will be more frequent, and more considerable, as the difference between the periods of the undulation and of the natural vibration is greater.

Corollary. It was long an established opinion, that heat consists in vibrations of the particles of bodies, and is capable of being transmitted by undulations through an apparent vacuum. (NEWT. Opt. Qu. 18.) This opinion has been of late very much abandoned. Count RUMFORD, Professor PICTET, and Mr. DAVY, are almost the only authors who have appeared to favour it; but it seems to have been rejected without any good grounds, and will probably very soon recover its popularity.

Let us suppose that these vibrations are less frequent than those of light; all bodies therefore are liable to permanent vibrations slower than those of light; and indeed almost all are liable to luminous vibrations, either when in a state of ignition, or in the circumstances of solar phosphori; but much less easily, and in a much less degree, than to the vibrations of heat. It will follow from these suppositions, that the more frequent luminous undulations will be more retarded than the less frequent; and

consequently, that blue light will be more refrangible than red, and radiant heat least of all; a consequence which coincides exactly with the highly interesting experiments of Dr. HERSCHEL. (Phil. Trans. for 1800. p. 284.) It may also be easily conceived, that the actual existence of a state of slower vibration may tend still more to retard the more frequent undulations, and that the refractive power of solid bodies may be sensibly increased by an increase of temperature, as it actually appears to have been in EULER's experiments. (Acad. de Berlin. 1762. p. 328.)

Scholium. If, notwithstanding, this proposition should appear to be insufficiently demonstrated, it must be allowed to be at least equally explanatory of the phenomena with any thing that can be advanced on the other side, from the doctrine of projectiles; since a supposed accelerating force must act in some other proportion than that of the bulk of the particles; and, if we call this an elective attraction, it is only veiling under a chemical term, our incapacity of assigning a mechanical cause. Mr. SHORT, when he found by observation the equality of the velocity of light of all colours, felt the objection so forcibly, that he immediately drew an inference from it in favour of the undulatory system. It is assumed in the proposition, that when light is dispersed by refraction, the corpuscles of the refracting substance are in a state of actual alternate motion, and contribute to its transmission; but it must be confessed, that we cannot at present form a very decided and accurate conception of the forces concerned in maintaining these corpuscular vibrations.

PROPOSITION VIII.

When two Undulations, from different Origins, coincide either perfectly or very nearly in Direction, their joint effect is a Combination of the Motions belonging to each.

Since every particle of the medium is affected by each undulation, wherever the directions coincide, the undulations can proceed no otherwise than by uniting their motions, so that the joint motion may be the sum or difference of the separate motions, accordingly as similar or dissimilar parts of the undulations are coincident.

I have, on a former occasion, insisted at large on the application of this principle to harmonics; (Phil. Trans. for 1800. p. 130.) and it will appear to be of still more extensive utility in explaining the phenomena of colours. The undulations which are now to be compared are those of equal frequency. When the two series coincide exactly in point of time, it is obvious that the united velocity of the particular motions must be greatest, and, in effect at least, double the separate velocities; and also, that it must be smallest, and if the undulations are of equal strength, totally destroyed, when the time of the greatest direct motion belonging to one undulation coincides with that of the greatest retrograde motion of the other. In intermediate states, the joint undulation will be of intermediate strength; but by what laws this intermediate strength must vary, cannot be determined without further data. It is well known that a similar cause produces in sound, that effect which is called a beat; two series of undulations of nearly equal magnitude co-operating and destroying each other alternately, as they coincide

more or less perfectly in the times of performing their respective motions.

COROLLARY 1. *Of the Colours of striated Surfaces.*

BOYLE appears to have been the first that observed the colours of scratches on polished surfaces. NEWTON has not noticed them. MAZEAS and Mr. BROUGHAM have made some experiments on the subject, yet without deriving any satisfactory conclusion. But all the varieties of these colours are very easily deduced from this proposition.

Let there be in a given plane two reflecting points very near each other, and let the plane be so situated that the reflected image of a luminous object seen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with respect to both points, considering them as capable of reflecting in all directions. Let one of the points be now depressed below the given plane; then the whole path of the light reflected from it, will be lengthened by a line which is to the depression of the point as twice the cosine of incidence to the radius. Fig. 2.

If, therefore, equal undulations of given dimensions be reflected from two points, situated near enough to appear to the eye but as one, wherever this line is equal to half the breadth of a whole undulation, the reflection from the depressed point will so interfere with the reflection from the fixed point, that the progressive motion of the one will coincide with the retrograde motion of the other, and they will both be destroyed; but, when this line is equal to the whole breadth of an undulation, the effect will be doubled; and when to a breadth and a half, again destroyed; and thus for a considerable number of alternations; and, if the reflected undulations be of different kinds, they will

be variously affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of retardation.

In order that the effect may be the more perceptible, a number of pairs of points must be united into two parallel lines; and, if several such pairs of lines be placed near each other, they will facilitate the observation. If one of the lines be made to revolve round the other as an axis, the depression below the given plane will be as the sine of the inclination; and, while the eye and luminous object remain fixed, the difference of the length of the paths will vary as this sine.

The best subjects for the experiment are Mr. COVENTRY's exquisite micrometers; such of them as consist of parallel lines drawn on glass, at the distance of one five hundredth of an inch, are the most convenient. Each of these lines appears under a microscope to consist of two or more finer lines, exactly parallel, and at the distance of somewhat more than a twentieth of that of the adjacent lines. I placed one of these so as to reflect the sun's light at an angle of 45°, and fixed it in such a manner, that while it revolved round one of the lines as an axis, I could measure its angular motion; and I found, that the brightest red colour occurred at the inclinations $10\frac{1}{4}°$, $20\frac{3}{4}°$, 32°, and 45°; of which the sines are as the numbers 1, 2, 3, and 4. At all other angles also, when the sun's light was reflected from the surface, the colour vanished with the inclination, and was equal at equal inclinations on either side.

This experiment affords a very strong confirmation of the theory. It is impossible to deduce any explanation of it from any hypothesis hitherto advanced; and I believe it would be

difficult to invent any other that would account for it. There is a striking analogy between this separation of colours, and the production of a musical note by successive echoes from equidistant iron palisades; which I have found to correspond pretty accurately with the known velocity of sound, and the distances of the surfaces.

It is not improbable that the colours of the integuments of some insects, and of some other natural bodies, exhibiting in different lights the most beautiful versatility, may be found to be of this description, and not to be derived from thin plates. In some cases, a single scratch or furrow may produce similar effects, by the reflection of its opposite edges.

COROLLARY II. *Of the Colours of thin Plates.*

When a beam of light falls on two parallel refracting surfaces, the partial reflections coincide perfectly in direction; and, in this case, the interval of retardation, taken between the surfaces, is to their distance as twice the cosine of the angle of refraction to the radius. For, in Fig. 3, drawing AB and CD perpendicular to the rays, the times of passing through BC and AD will be equal, and DE will be half the interval of retardation; but DE is to CE as the sine of DCE to the radius. Hence, that DE may be constant, or that the same colour may be reflected, the thickness CE must vary as the secant of the angle of refraction CED: which agrees exactly with NEWTON's experiments; for the correction is perfectly inconsiderable.

Let the medium between the surfaces be rarer than the surrounding mediums; then the impulse reflected at the second surface, meeting a subsequent undulation at the first, will render the particles of the rarer medium capable of wholly stopping

the motion of the denser, and destroying the reflection, (PROP. IV.) while they themselves will be more strongly propelled than if they had been at rest; and the transmitted light will be increased. So that the colours by reflection will be destroyed, and those by transmission rendered more vivid, when the double thicknesses, or intervals of retardation, are any multiples of the whole breadths of the undulations; and, at intermediate thicknesses the effects will be reversed; according to the NEWTONIAN observations.

If the same proportions be found to hold good with respect to thin plates of a denser medium, which is indeed not improbable, it will be necessary to adopt the corrected demonstration of PROP. IV. but, at any rate, if a thin plate be interposed between a rarer and a denser medium, the colours by reflection and transmission may be expected to change places.

From NEWTON's measures of the thicknesses reflecting the different colours, the breadth and duration of their respective undulations may be very accurately determined; although it is not improbable, that when the glasses approach very near, the atmosphere of ether may produce some little irregularity. The whole visible spectrum appears to be comprised within the ratio of three to five, or a major sixth in music; and the undulations of red, yellow, and blue, to be related in magnitude as the numbers 8, 7, and 6; so that the interval from red to blue is a fourth. The absolute frequency expressed in numbers is too great to be distinctly conceived, but it may be better imagined by a comparison with sound. If a chord sounding the tenor $\bar{c}$, could be continually bisected 40 times, and should then vibrate, it would afford a yellow green light: this being denoted by $\overset{41}{c}$, the extreme red would be $\overset{40}{a}$, and the blue $\overset{41}{d}$.

The absolute length and frequency of each vibration is expressed in the table; supposing light to travel in 8½ minutes 500,000,000000 feet.

Colours.	Length of an Undulation in parts of an Inch, in Air.	Number of Undulations in an Inch.	Number of Undulations in a Second.
Extreme -	.0000266	37640	463 millions of millions
Red - -	.0000256	39180	482
Intermediate	.0000246	40720	501
Orange - -	.0000240	41610	512
Intermediate	.0000235	42510	523
Yellow -	.0000227	44000	542
Intermediate	.0000219	45600	561 (= 2" nearly)
Green - -	.0000211	47460	584
Intermediate	.0000203	49320	607
Blue - -	.0000196	51110	629
Intermediate	.0000189	52910	652
Indigo - -	.0000185	54070	665
Intermediate	.0000181	55240	680
Violet - -	.0000174	57490	707
Extreme - -	.0000167	59750	735

Scholium. It was not till I had satisfied myself respecting all these phenomena, that I found in Hooke's Micrographia, a passage which might have led me earlier to a similar conclusion. "It is most evident that the reflection from the under or fur-"ther side of the body, is the principal cause of the production "of these colours. — Let the ray fall obliquely on the thin "plate, part therefore is reflected back by the first superficies, "—part refracted to the second surface,—whence it is reflected "and refracted again.—So that, after two refractions and one

" reflection, there is propagated a kind of fainter ray—," and, " by reason of the time spent in passing and repassing,—this " fainter pulse comes behind the" former reflected " pulse; so " that hereby, (the surfaces being so near together that the eye " cannot discriminate them from one,) this confused or duplicated " pulse, whose strongest part preecedes, and whose weakest fol- " lows, does produce on the retina, the sensation of a yellow. " If these surfaces are further removed asunder, the weaker " pulse may become coincident with the" reflection of the " second," or next following pulse, from the first surface, " and " lagg behind that also, and be coincident with the third, " fourth, fifth, sixth, seventh, or eighth—; so that, if there be " a thin transparent body, that from the greatest thinness requi- " site to produce colours, does by degrees grow to the greatest " thickness,—the colours shall be so often repeated, as the " weaker pulse does lose paces with its primary or first pulse, " and is coincident with a" subsequent " pulse. And this, as " it is coincident, or follows from the first hypothesis I took of " colours, so upon experiment have I found it in multitudes of " instances that seem to prove it." (P. 65—67.) This was printed about seven years before any of NEWTON's experiments were made. We are informed by NEWTON, that HOOKE was afterwards disposed to adopt his " suggestion" of the nature of colours; and yet it does not appear that HOOKE ever applied that improvement to his explanation of these phenomena, or inquired into the necessary consequence of a change of obliquity, upon his original supposition, otherwise he could not but have discovered a striking coincidence with the measures laid down by NEWTON from experiment. All former attempts to explain the colours of thin plates, have either proceeded on suppositions

which, like NEWTON's, would lead us to expect the greatest irregularities in the direction of the refracted rays; or, like Mr. MICHELL's, would require such effects from the change of the angle of incidence, as are contrary to the effects observed; or they are equally deficient with respect to both these circumstances, and are inconsistent with the most moderate attention to the principal phenomena.

COROLLARY III. *Of the Colours of thick Plates.*

When a beam of light passes through a refracting surface, especially if imperfectly polished, a portion of it is irregularly scattered, and makes the surface visible in all directions, but most conspicuously in directions not far distant from that of the light itself: and, if a reflecting surface be placed parallel to the refracting surface, this scattered light, as well as the principal beam, will be reflected, and there will also be a new dissipation of light, at the return of the beam through the refracting surface. These two portions of scattered light will coincide in direction; and, if the surfaces be of such a form as to collect the similar effects, will exhibit rings of colours. The interval of retardation is here, the difference between the paths of the principal beam and of the scattered light between the two surfaces; of course, wherever the inclination of the scattered light is equal to that of the beam, although in different planes, the interval will vanish, and all the undulations will conspire. At other inclinations, the interval will be the difference of the secants from the secant of the inclination or angle of refraction of the principal beam. From these causes, all the colours of concave mirrors observed by NEWTON and others are necessary consequences: and it appears that their production, though

somewhat similar, is by no means, as NEWTON imagined, identical with the production of those of thin plates.

COROLLARY IV. *Of Blackness.*

In the three preceding corollaries, we have considered the refracting and reflecting substances as limited by a mathematical surface; but this is perhaps never physically true. The ethereal atmospheres may extend on each side the surface as far as the breadth of one or more undulations; and, if they be supposed to vary equally in density at every part, the partial reflections from each of the infinite number of surfaces, where the density changes, will very much interfere with each other, and destroy a considerable portion of the reflected light, so that the substance may become positively black; and this effect may take place in a greater or less degree, as the density of the ethereal atmosphere varies more or less equably; and, in some cases, particular undulations being more affected than others, a tinge of colour may be produced. Accordingly, M. BOUGUER has observed a considerable loss of light, and in some instances a tinge of colour, in total reflections at the surface of a rarer medium.

COROLLARY V. *Of Colours by Inflection.*

Whatever may be the cause of the inflection of light passing through a small aperture, the light nearest its centre must be the least diverted, and the nearest to its sides the most: another portion of light falling very obliquely on the margin of the aperture, will be copiously reflected in various directions; some of which will either perfectly or very nearly coincide in direction with the unreflected light, and, having taken a circuitous

route, will so interfere with it, as to cause an appearance of colours. The length of the two tracks will differ the less, as the direction of the reflected light has been less changed by its reflection, that is, in the light passing nearest to the margin; so that the blues will appear in the light nearest the shadow. The effect will be increased and modified, when the reflected light falls within the influence of the opposite edge, so as to interfere with the light simply inflected by that also.

But, in order to examine the consequences more minutely, it will be convenient to suppose the inflection caused by an ethereal atmosphere, of a density varying as a given power of the distance from a centre, as in the eighth proposition of the last BAKERIAN Lecture. (Phil. Trans. for 1801, p. 83.) Putting $r = 3$, and $x = \frac{1}{2}$, I have constructed a diagram, (Fig. 4.) which shows, by the two pairs of curves, the relative position of the reflected and unreflected portions of any one undulation at two successive times, and also, by shaded lines drawn across, the parts where the intervals of retardation are in arithmetical progression, and where similar colours will be exhibited at different distances from the inflecting substance. The result fully agrees with the observations of NEWTON's third book, and with those of later writers. But I do not consider it as quite certain, until further experiments have been made on the inflecting power of different substances, that Dr. HOOKE's explanation of inflection, by the tendency of light to diverge, may not have some pretensions to truth. I am sorry to be obliged to recall here the assent which, at first sight, I was induced to give to a supposed improvement of a late author. (Phil. Trans. for 1800, p. 128.)

Scholium. In the construction of the diagram, it becomes necessary to find the time spent by each ray in its passage.

Since the velocity was denoted by $x^{-\frac{1}{r}}$, on the supposition of a projectile, it will be as $x^{\frac{1}{r}}$ on the contrary supposition, (Phil. Trans. for 1801, p. 27. *Schol.* 2. Prop. I.) and the fluxion of the distance described being $\frac{\dot{x}}{\sqrt{1-yy}}$, that of the time will be $\frac{x^{-\frac{1}{r}}\dot{x}}{\sqrt{1-yy}}$ or $\frac{rs}{1-r}\cdot\frac{\dot{y}}{yy\cdot\sqrt{1-yy}}$, of which the fluent is $\frac{r}{1-r}\cdot\frac{s}{y}\cdot\sqrt{1-yy}$. Therefore, with the radius $x^{1-\frac{1}{r}}$, describe a circle concentric with the surfaces of the inflecting atmosphere, then the angle described by the ray during its passage through the atmosphere, will always be to the angle subtended by the line cut off by this circle from the incident ray produced, in the ratio of r to $r-1$; and the time spent in this passage, will be in the same ratio to the time that would have been spent in describing this intercepted portion with the initial velocity. For y, being equal to $sx^{\frac{1}{r}-1}$, is the sine of the inclination of the incident ray to the radius, where it meets this circle; therefore, by the proposition quoted, the angle described is in a given ratio to the angle at the centre, which is the difference of the inclinations. Making $x^{1-\frac{1}{r}}$ or $\frac{s}{y}$ radius, the sine, instead of y, becomes s, and the cosine $\sqrt{\frac{ss}{yy}-ss}$, or $\frac{s}{y}\sqrt{1-yy}$, and, when $y=ss$, $\sqrt{1-ss}$; therefore the line intercepted is to the difference of the fluents as r to $r-1$. (See also YOUNG'S Syllabus, Art. 372.)

PROPOSITION IX.

Radiant Light consists in Undulations of the luminiferous Ether.

This proposition is the general conclusion from all the preceding; and it is conceived that they conspire to prove it in as satisfactory a manner as can possibly be expected from the

nature of the subject. It is clearly granted by NEWTON, that there are undulations, yet he denies that they constitute light; but it is shown in the three first Corollaries of the last Proposition, that all cases of the increase or diminution of light are referable to an increase or diminution of such undulations, and that all the affections to which the undulations would be liable, are distinctly visible in the phenomena of light; it may therefore be very logically inferred, that the undulations are light.

A few detached remarks will serve to obviate some objections which may be raised against this theory.

1. NEWTON has advanced the singular refraction of the Iceland crystal, as an argument that the particles of light must be projected corpuscles; since he thinks it probable that the different sides of these particles must be differently attracted by the crystal, and since HUYGENS has confessed his inability to account in a satisfactory manner for all the phenomena. But, contrarily to what might have been expected from NEWTON's usual accuracy and candour, he has laid down a new law for the refraction, without giving a reason for rejecting that of HUYGENS, which Mr. HAUY has found to be more accurate than NEWTON's; and, without attempting to deduce from his own system any explanation of the more universal and striking effects of doubling spars, he has omitted to observe that HUYGENS's most elegant and ingenious theory perfectly accords with these general effects, in all particulars, and of course derives from them additional pretensions to truth: this he omits, in order to point out a difficulty, for which only a verbal solution can be found in his own theory, and which will probably long remain unexplained by any other.

2. Mr. MICHELL has made some experiments, which appear to show that the rays of light have an actual momentum, by

means of which a motion is produced when they fall on a thin plate of copper delicately suspended. (PRIESTLEY's Optics.) But, taking for granted the exact perpendicularity of the plate, and the absence of any ascending current of air, yet since, in every such experiment, a greater quantity of heat must be communicated to the air at the surface on which the light falls than at the opposite surface, the excess of expansion must necessarily produce an excess of pressure on the first surface, and a very perceptible recession of the plate in the direction of the light. Mr. BENNET has repeated the experiment, with a much more sensible apparatus, and also in the absence of air; and very justly infers from its total failure, an argument in favour of the undulatory system of light. (Phil. Trans. for 1792, p. 87.) For, granting the utmost imaginable subtility of the corpuscles of light, their effects might naturally be expected to bear some proportion to the effects of the much less rapid motions of the electrical fluid, which are so very easily perceptible, even in their weakest states.

9. There are some phenomena of the light of solar phosphori, which at first sight might seem to favour the corpuscular system; for instance, its remaining many months as if in a latent state, and its subsequent re-emission by the action of heat. But, on further consideration, there is no difficulty in supposing the particles of the phosphori which have been made to vibrate by the action of light, to have this action abruptly suspended by the intervention of cold, whether as contracting the bulk of the substance or otherwise; and again, after the restraint is removed, to proceed in their motion, as a spring would do which had been held fast for a time in an intermediate stage of its vibration; nor is it impossible that heat itself may, in some circumstances, become in a similar manner latent. (NICHOLSON's

Journal. Vol. II. p. 399.) But the affections of heat may perhaps hereafter be rendered more intelligible to us; at present, it seems highly probable that light differs from heat only in the frequency of its undulations or vibrations; those undulations which are within certain limits, with respect to frequency, being capable of affecting the optic nerve, and constituting light; and those which are slower, and probably stronger, constituting heat only; that light and heat occur to us, each in two predicaments, the vibratory or permanent, and the undulatory or transient state; vibratory light being the minute motion of ignited bodies, or of solar phosphori, and undulatory or radiant light the motion of the ethereal medium excited by these vibrations; vibratory heat being a motion to which all material substances are liable, and which is more or less permanent; and undulatory heat that motion of the same ethereal medium, which has been shown by Mr. KING, (Morsels of Criticism. 1786. p. 99,) and M. PICTET, (*Essais de Physique.* 1790,) to be as capable of reflection as light, and by Dr. HERSCHEL to be capable of separate refraction. (Phil. Trans. for 1800. p. 284.) How much more readily heat is communicated by the free access of colder substances, than either by radiation or by transmission through a quiescent medium, has been shown by the valuable experiments of Count RUMFORD. It is easy to conceive that some substances, permeable to light, may be unfit for the transmission of heat, in the same manner as particular substances may transmit some kinds of light, while they are opaque with respect to others.

On the whole it appears, that the few optical phenomena which admit of explanation by the corpuscular system, are equally consistent with this theory; that many others, which have long been known, but never understood, become by these means perfectly intelligible; and that several new facts are

found to be thus only reducible to a perfect analogy with other facts, and to the simple principles of the undulatory system. It is presumed, that henceforth the second and third books of NEWTON's Optics will be considered as more fully understood than the first has hitherto been; but, if it should appear to impartial judges, that additional evidence is wanting for the establishment of the theory, it will be easy to enter more minutely into the details of various experiments, and to show the insuperable difficulties attending the NEWTONIAN doctrines, which, without necessity, it would be tedious and invidious to enumerate. The merits of their author in natural philosophy, are great beyond all contest or comparison; his optical discovery of the composition of white light, would alone have immortalised his name; and the very arguments which tend to overthrow his system, give the strongest proofs of the admirable accuracy of his experiments.

Sufficient and decisive as these arguments appear, it cannot be superfluous to seek for further confirmation; which may with considerable confidence be expected, from an experiment very ingeniously suggested by Professor ROBISON, on the refraction of the light returning to us from the opposite margins of Saturn's ring; for, on the corpuscular theory, the ring must be considerably distorted when viewed through an achromatic prism: a similar distortion ought also to be observed in the disc of Jupiter; but, if it be found that an equal deviation is produced in the whole light reflected from these planets, there can scarcely be any remaining hope to explain the affections of light, by a comparison with the motions of projectiles.

Fig. 1.

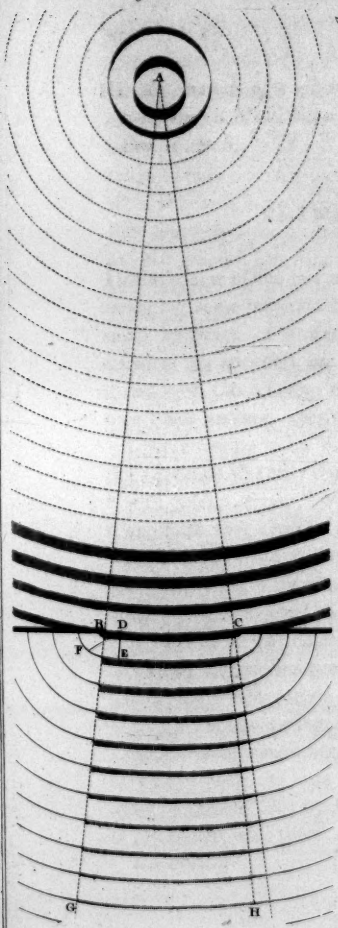
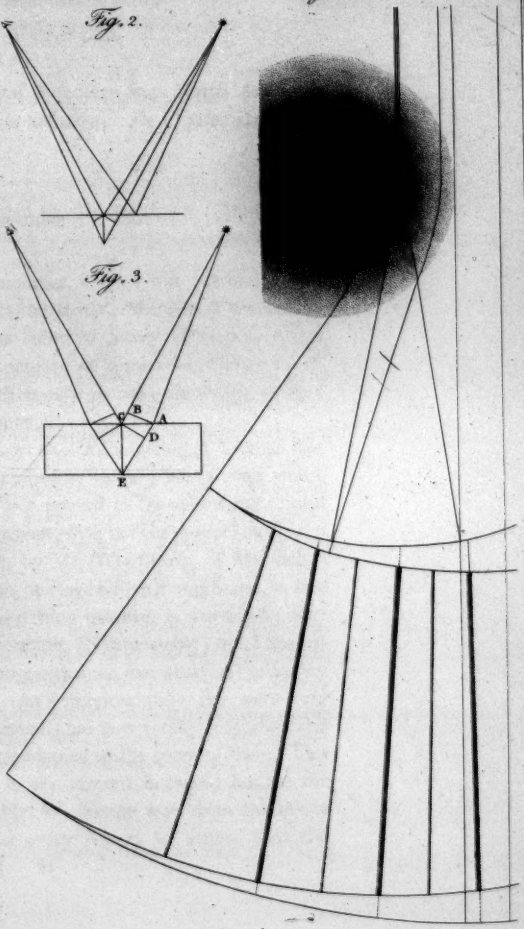


Fig. 2.

Fig. 3.

Fig. 4.



III. *An Analysis of a mineral Substance from* North America, *containing a Metal hitherto unknown.* *By* Charles Hatchett, *Esq. F. R. S.*

Read November 26, 1801.

In the course of the last summer, when I was examining and arranging some minerals in the British Museum, I observed a small specimen of a dark-coloured heavy substance, which attracted my attention, on account of some resemblance which it had with the Siberian chromate of iron, on which at that time I was making experiments.

Upon referring to Sir Hans Sloane's catalogue, I found that this specimen was only described as "a very heavy black stone, "with golden streaks," which proved to be yellow mica; and it appeared, that it had been sent, with various specimens of iron ores, to Sir Hans Sloane, by Mr. Winthrop, of Massachusets. The name of the mine, or place where it was found, is also noted in the catalogue; the writing however is scarcely legible: it appears to be an Indian name, (Nautneauge;) but I am informed by several American gentlemen, that many of the Indian names (by which certain small districts, hills, &c. were forty or fifty years ago distinguished,) are now totally forgotten, and European names have been adopted in the room of them. This may have been the case in the present instance; but, as the other specimens sent by Mr. Winthrop were from the mines of Massachusets, there is every reason to believe that the

mineral substance in question came from one of them, although it may not now be easy to identify the particular mine.

§ I. DESCRIPTION OF THE ORE.

The external colour is dark brownish gray.

The internal colour is the same, inclining to iron gray.

The longitudinal fracture is imperfectly lamellated; and the cross fracture shews a fine grain.

The lustre is vitreous, slightly inclining in some parts to metallic lustre.

It is moderately hard, and is very brittle.

The colour of the streak or powder is dark chocolate brown.

The particles are not attracted by the magnet.

The specific gravity, at temp. 65°, is 5918.*

Experiment 1.

Some of the ore, reduced to fine powder, was digested in boiling muriatic acid for about one hour.

The acid appeared to have acted but slightly upon the powder; as the former remained colourless, and the latter did not seem to be diminished. A portion, however, chiefly of iron, was found to be dissolved; for ammonia formed a yellow flocculent precipitate; prussiate of potash produced one which was blue;

* The following results of some experiments which I have purposely made, will shew how much the specific gravity of this ore is different from that of Wolfram, and Siberian chromate of iron.

Pure Wolfram, free from extraneous substances, at temp. 65° - -	6955.
Siberian chromate of iron, containing some of the green oxide -	3728.
Pure Siberian chromate of iron - - -	4355.

The Siberian chromate of iron, like all other mineral substances which are not crystallized, and which consequently are not always homogeneous, must evidently be liable to considerable variations in specific gravity.

and tincture of galls, when the excess of acid had been previously saturated by an alkali, formed a precipitate of a rich purplish brown colour.

Experiment II.

Another portion of the powder was, in like manner, digested with nitric acid; but, excepting some slight traces of iron, this acid afforded nothing worthy of notice; the action of it upon the ore, was indeed scarcely perceptible.

Experiment III.

Some of the pulverized ore was digested with concentrated sulphuric acid, in a strongly-heated sand-bath, until nearly the whole of the acid was evaporated; the edges of the mass then appeared bluish, and became white, when boiling distilled water was added.

This acid certainly acted much more powerfully than those which have been mentioned; but still only a small part of the ore was dissolved. It must however be observed, that a very copious blue precipitate was obtained by prussiate of potash; a plentiful purplish brown precipitate was also produced by tincture of galls, after the excess of acid had been saturated by an alkali; and, lastly, when the yellow ferruginous precipitate formed by ammonia was dissolved in diluted nitric acid, some white flocculi remained, which were completely insoluble in the acid, even when it was added so as to be in considerable excess.

From these experiments it was evident, that the ore could not readily be decomposed by the direct application of the mineral acids; and I therefore had recourse to the following

method, which has frequently been employed with success in similar cases.

ANALYSIS.

A.

A mixture of 200 grains of the powdered ore with five times the weight of carbonate of potash, was exposed to a strong red heat, in a silver crucible. As soon as the matter began to flow, a very perceptible effervescence took place; and, when this had subsided, the whole was poured into a proper vessel.

The mass, when cold, was grayish-brown.

Boiling distilled water was poured upon it; and the brown residuum, which was considerable, was well edulcorated upon a filter.

The filtrated liquor had a slight yellowish tinge, and, being supersaturated with nitric acid, afforded a copious white flocculent precipitate, which speedily subsided; but, although a very considerable additional quantity of nitric acid was poured upon the precipitate, it was not re-dissolved.

The residuum of the ore was dark brown, and was again melted with potash, and treated as before; but scarcely any effect was thus produced; the alkali was therefore washed off, and the powder was digested with muriatic acid, which soon assumed the deep yellow colour usually communicated to it by iron. After half an hour, the acid was decanted, and the residuum was washed with distilled water.

This powder was now of a much paler colour; and, being mixed with potash, it was melted and treated as before. A considerable precipitate was again obtained by the addition of nitric acid; and the residuum, after being digested with muriatic acid, was again fused with potash, by which means the

whole was completely decomposed, after about five repetitions of each operation.

B.

The muriatic solution was diluted, and, being saturated with ammonia, afforded a plentiful ochraceous precipitate; which again was dissolved in cold dilute nitric acid, and afforded a small quantity of a white insoluble substance, similar to that which was obtained from the alkaline solution. From this nitric solution, I then obtained, by means of ammonia, a precipitate of oxide of iron, which, being properly dried, weighed 40 grains.

C.

The different alkaline solutions which had been made subsequent to that which has been first mentioned, were mixed together, and, being supersaturated with nitric acid, afforded the same white insoluble precipitate; the total quantity of which, obtained from 200 grains of the ore, amounted to about 155 grains.

The liquor from which this precipitate had been separated by nitric acid, was then saturated with ammonia, and, being boiled, afforded about 2 grains of oxide of iron.

I obtained, therefore, from 200 grains of the ore,

	Grains.	Grains.
Oxide of iron - - - -	42	
And of the white precipitated substance	155	= 197.

But, as I could not repeat the analysis without destroying the remaining part of the only specimen at present known of this ore, I do not wish the above stated proportions to be regarded as rigidly exact; it will be sufficient, therefore, to say at present, that the ore is composed of about three parts of the white matter, and rather less than one of iron.

§ II. PROPERTIES OF THE WHITE PRECIPITATE.

A.

It is of a pure white, and is not extremely heavy.

It has scarcely any perceptible flavour, nor does it appear to be soluble in boiling water; when, however, some of the powder is placed upon litmus paper moistened with distilled water, the paper in a few minutes evidently becomes red.

B.

1. When examined by the blow-pipe, it is not fusible per se in a spoon of platina, nor upon charcoal, but only becomes of a less brilliant white.

2. Borax does not appear to act upon it; for the white particles are only dispersed throughout the globule.

3. It produces an effervescence when fused with carbonate of soda, and forms a colourless salt; but, if too much of it be added, then the mass, when cold, appears like a white opaque enamel.

4. When carbonate of potash is employed, the effects are similar in every respect to those of soda; and it may here be remarked, that the saline combinations thus formed with soda, or potash, are soluble in water; and that these solutions have the same properties as that which was formed when the ore was decomposed by an alkali. The portion of the white precipitate which may be in excess, subsides unaltered, when the globules are dissolved in water.

5. Phosphate of ammonia produces a very marked effect; for, when melted in a platina spoon, if some of the white substance be added, a considerable effervescence takes place, and the two substances rapidly unite. The globule, when cold, is

deep blue, with a tinge of purple, but, when held between the eye and the light, it appears of a greenish gray colour.

C.

It is perfectly insoluble, and remains unchanged in colour, and in every other respect, when digested in boiling concentrated nitric acid.

D.

It is dissolved by boiling sulphuric acid, and forms a transparent colourless solution, which is however only permanent while the acid remains in a concentrated state; for, if a large quantity of water be added to the solution, or if the latter be poured into a vessel of distilled water, the whole in a few minutes assumes a milky appearance, and a white precipitate is gradually deposited, which cracks as it becomes dry upon the filter, and, from white, changes to a lavender-blue colour, and again, when completely dry, to a brownish gray. It is then insoluble in water, has not any flavour, is semi-transparent, and breaks with a glossy vitreous fracture.

This substance is much heavier than the original white precipitate; and in a very slight degree may be dissolved by boiling muriatic acid, or by boiling lixivium of potash.

Upon examining these solutions, I found that both contained the original white substance, together with some sulphuric acid; so that the precipitate obtained from the sulphuric solution by the addition of water, is a sulphate of the white matter.*

The whole is not however precipitated by water; for a part

* This sulphate is also precipitated when the sulphuric solution has been long exposed in an open vessel to the air; and, according as this may be moist or dry, the effect is produced sooner or later.

remains in solution, which may be separated from the sulphuric acid by either of the fixed alkalis, or by ammonia.

The sulphuric solution is not rendered turbid by the addition of water, until some minutes at least have elapsed; when, therefore, some prussiate of potash was added immediately after the water, the colour of the liquor became olive green, and a copious precipitate, of a beautiful olive colour, was gradually deposited.

Tincture of galls, after a few minutes, caused the liquor to become turbid, and a very high orange-coloured precipitate was obtained.

A few drops of phosphoric acid were added to a part of the concentrated sulphuric solution; and, after about 12 hours, the whole became a white opaque stiff jelly, which was insoluble in water.

Potash, soda, and ammonia, whether pure or in the state of carbonates, separate the substance in question from the sulphuric solution, in the form of a white flocculent precipitate; and, when these alkalis are added to a considerable excess, they do not redissolve the precipitate, unless they are heated; then, indeed, the fixed alkalis act upon it, and form combinations which have already been mentioned, but which we shall soon have occasion more particularly to notice.

E.

1. The white precipitate, when recently separated from potash, is soluble in boiling muriatic acid; and this solution may be considerably diluted with water, without any change being produced.

2. A part was evaporated to dryness, and left a pale yellow substance, which was not soluble in water, and was dissolved

with great difficulty, when it was again digested with muriatic acid.

3. Prussiate of potash changed the colour of the muriatic solution to an olive-green; the liquor then gradually became turbid, and an olive-coloured precipitate was obtained, similar to that which has been lately mentioned. But,

4. If some nitric acid was previously added to the muriatic solution, then the prussiate changed the liquor to a grass-green, but did not produce any precipitate.

5. Tincture of galls, in a few minutes, formed an orange-coloured precipitate, like that which has been mentioned; but, if the acid was in too great an excess, it was necessary to add a small quantity of lixivium of potash or soda, before the precipitate could be obtained.

6. A small quantity of phosphoric acid, being added to the muriatic solution, in a few hours formed a white flocculent precipitate.

7. Potash, soda, and ammonia, also produced white flocculent precipitates, which were not redissolved by an excess of the alkalis, unless the liquors were heated; and, in that case, part was dissolved by the fixed alkalis, but not by ammonia.

8. The muriatic solution did not yield any precipitate, when the muriates of lime, magnesia, and strontian, were added; but muriate of barytes formed a slight cloud.

9. When a piece of zinc was immersed in the muriatic solution, a white flocculent precipitate was obtained.*

* This appears to indicate the obstinacy with which this substance retains a certain portion of oxygen; for we here see that zinc does not precipitate it in the metallic state, but only reduces it to an insoluble oxide.

F.

The acetous acid has not any apparent effect on the white precipitate, when long digested with it.

G.

The fixed alkalis readily combine with this substance, both in the dry and in the humid way.

We have already seen, that the former method was employed with success in the analysis of the ore; and the experiments made with the blow-pipe may be regarded as an additional confirmation. In each of these cases, the white precipitate combined with the alkali, as soon as the heat was sufficient to cause the latter to flow; and, when a carbonate was employed, a portion of carbonic acid was expelled.

The carbonic acid was in like manner disengaged, when the white precipitate was boiled with lixivium of carbonate of potash, or of soda; and the solutions thus prepared, resembled in every respect those which were formed by dissolving in water the salts which had been produced in the dry way.

It will be proper here to give a more particular account of these combinations.

1. Some of the white precipitate was digested, during nearly one hour, with boiling lixivium of pure or caustic potash: about one-fourth of the powder was dissolved; and the remainder, which appeared little if at all altered, subsided to the bottom of the vessel.

The clear solution, which contained a great excess of alkali, was decanted; and, by gentle evaporation, yielded a white glittering salt, in scales, very much resembling the concrete boracic acid.

The salt was placed upon a filter, so that the lixivium might be separated. It was then washed with a small quantity of cold distilled water; and, being dried, remained as above described, although constantly exposed to the open air.

This salt had an acrid disagreeable flavour, and contained a small excess of alkali. It did not dissolve very readily in cold water; but, when dissolved, the solution was perfect and permanent.

Some nitric acid was added to part of the solution, and immediately rendered it white and turbid. In a short time, a white precipitate was collected, similar to that which had been employed to neutralise the potash; and the clear supernatant liquor, being evaporated, only afforded nitre.

Prussiate of potash was added to another portion; but did not produce any effect, until some muriatic acid was dropped into the liquor, which then immediately assumed a tinge of olive green, and slowly deposited a precipitate of the same colour.

Tincture of galls did not affect the solution at first; but, when a few drops of muriatic acid had been added, it gradually lost its transparency, and yielded an orange-coloured precipitate.

2. As so large a part of the white precipitate had remained undissolved in the foregoing experiment, it was digested again with another portion of the same lixivium, but without any effect. I therefore washed off the alkali, and boiled some nitric acid with the powder, until the acid was completely evaporated. After this, the powder was exposed to a strong heat in a sand-bath. It was then again digested with the lixivium, and a part was dissolved as before; but still the residuum required to be treated with nitric acid, before the alkaline liquor could again act upon it; so that it was necessary to repeat these alternate

operations several times, before the whole of the powder could be united with the alkali.

3. When the white precipitate was digested with solution of carbonate of potash, or of soda, it was dissolved, much in the same manner as above related; and the properties of the solutions, when examined by reagents, were also similar, excepting that the orange-coloured precipitates produced by tincture of galls were of a paler colour.

Tungstate of potash, molybdate of potash, and cobaltate of ammonia, being severally added to the solution of the white substance in potash, produced white flocculent precipitates.

Hydro-sulphuret of ammonia produced a reddish chocolate-coloured precipitate.

4. As the ore was decomposed by being fused with potash, the following experiment affords a curious instance (among the many already known) of the change in the order of affinities produced by a difference of temperature.

Some of the solution of the white precipitate in potash, was poured into the alkaline solution of iron, which was formerly known by the name of STAHL's *Tinctura Alkalina Martis*. Potash was in excess in both of these solutions; but nevertheless a cloud was immediately produced, and a brown ferruginous precipitate was deposited.

Part of this precipitate was dissolved in muriatic acid; and the solution, being examined in the usual way, yielded a blue precipitate when prussiate of potash was added, and a purplish brown precipitate with tincture of galls.

The other part of the precipitate was digested with dilute nitric acid; which dissolved the ferruginous part, but left untouched a white flocculent matter, perfectly resembling the

substance which has been so often mentioned. The precipitate therefore produced by the mixture of the two alkaline solutions, was a combination of the white matter with oxide of iron, very similar to the original ore.

H.

The white precipitate, when distilled with four parts of sulphur, remained pulverulent, and, from white, was only changed to a pale ash colour.

Nitric acid was digested on the powder, and, being heated, afforded some nitrous gas; after this, the powder became white, and in every respect recovered its original properties.

I.

Before I conclude this section, I must observe, that when the olive-green precipitates, obtained by prussiate of potash, were digested in an alkaline lixivium, they were decomposed; for the alkali combined with the prussic acid, and with a small part of the white matter; but the greater part of the latter remained undissolved, in the same white flocculent state which was noticed when the alkaline combinations were mentioned.

The orange-coloured precipitates, formed by tincture of galls, were also decomposed when digested in boiling nitric acid; and the white matter was recovered in its original state.

§ III. REMARKS.

The preceding experiments shew, that the ore which has been analysed, consists of iron combined with an unknown substance, and that the latter constitutes more than three-fourths of the whole. This substance is proved to be of a metallic nature, by the coloured precipitates which it forms with prussiate of potash, and with tincture of galls; by the effects which zinc

produces, when immersed in the acid solutions; and by the colour which it communicates to phosphate of ammonia, or rather to concrete phosphoric acid, when melted with it.

Moreover, from the experiments made with the blow-pipe, it seems to be one of those metallic substances which retain oxygen with great obstinacy, and are therefore of difficult reduction.

It is an acidifiable metal; for the oxide reddens litmus paper, expels carbonic acid, and forms combinations with the fixed alkalis. But it is very different from the acidifiable metals which have of late been discovered; for,

1. It remains white when digested with nitric acid.

2. It is soluble in the sulphuric and muriatic acids, and forms colourless solutions, from which it may be precipitated, in the state of a white flocculent oxide, by zinc, by the fixed alkalis, and by ammonia. Water also precipitates it from the sulphuric solution, in the state of a sulphate.

3. Prussiate of potash produces a copious and beautiful olive-green precipitate.

4. Tincture of galls forms orange or deep yellow precipitates.

5. Unlike the other metallic acids, it refuses to unite with ammonia.

6. When mixed and distilled with sulphur, it does not combine with it so as to form a metallic sulphuret.

7. It does not tinge any of the fluxes, except phosphoric acid, with which, even in the humid way, it appears to have a very great affinity.

8. When combined with potash and dissolved in water, it forms precipitates, upon being added to solutions of tungstate of potash, molybdate of potash, cobaltate of ammonia, and the alkaline solution of iron.

These properties completely distinguish it from the other

acidifiable metals, *viz.* arsenic, tungsten, molybdena, and chromium; as to the other metals lately discovered, such as uranium, titanium, and tellurium, they are still farther removed from it.

The colours of the precipitates produced by prussiate of potash and tincture of galls, approach the nearest to those afforded by titanium. But the prussiate of the latter is much browner; and the gallate is not of an orange colour, but of a brownish red, inclining to the colour of blood. Besides, even if these precipitates were more like each other, still the obstinacy with which titanium refuses to unite with the fixed alkalis, and the insolubility of it in acids when heated, sufficiently denote the different nature of these two substances.

The iron in the ore which has been examined, is apparently in the same state as it is in wolfram, *viz.* brown oxide; and this oxide is mineralised by the metallic acid which has been described, in the same manner as the oxides of iron and manganese are mineralised by the tungstic acid or rather oxide. For, from several experiments made upon a large scale, I have reason to believe that in wolfram, the tungsten has not attained the maximum of oxidation. Several facts in the course of the experiments lately described, seem to prove, that this new metal differs from tungsten and the other acidifiable metals, by a more limited extent of oxidation; for, unlike these, it seems to be incapable of retaining oxygen sufficient to enable the total quantity to combine with the fixed alkalis. In § II. G. 2, this is very evident; for, from the experiment there described it appears, that when the metallic acid or oxide was digested with lixivium of potash, only a part was dissolved; and that the remainder was insoluble in the same lixivium, till it had received

an additional portion of oxygen, by being treated with nitric acid; also that several of these alternate operations were required, before any given quantity of the metallic oxide could be completely combined with the alkali. Now there is much reason to believe, that in this case, when the metallic oxide or acid was digested with potash, the portion which was dissolved, received an accession of oxygen at the expense of the other part, which of course was thus reduced to the state of an insoluble oxide, and therefore required to be again oxidated by nitric acid, before it could combine with the alkaline solution; but still it appeared, that an adequate proportion of oxygen could never be superinduced, so as to render the oxide totally and immediately soluble in the alkalis by one operation, or even by two.

We may, therefore, regard this as an instance of the effects resulting from disposing affinity, and as very similar to those observed in respect to copper, which have been noticed by my ingenious friend Mr. CHENEVIX, in his valuable analysis of the arseniates of copper and of iron.*

My researches into the properties of this metal, have of course been much limited by the smallness of the quantity which I had to operate upon; but I flatter myself that more of the ore may soon be procured from the Massachuset mines, particularly as a gentleman now in England, (Mr. SMITH, Secretary to the American Philosophical Society,) has obligingly offered his assistance on this occasion. We shall then be able more fully to investigate the nature of this substance; and shall be more capable of judging how far it may be applicable to useful purposes. At present, all that can be said is, that the olive green prussiate and the orange-coloured gallate are fine colours;

* Phil. Trans. for 1801, p. 233.

and, as they do not appear to fade when exposed to light and air, they might probably be employed with advantage as pigments.

I am much inclined to believe, that the time is perhaps not very distant, when some of the newly-discovered metals, and other substances, which are now considered as simple, primitive, and distinct bodies, will be found to be compounds. Yet I only entertain and state this opinion as a probability; for, until an advanced state of chemical knowledge shall enable us to compose, or at least to decompose, these bodies, each must be classed and denominated as a substance *sui generis*. Considering, therefore, that the metal which has been examined is so very different from those hitherto discovered, it appeared proper that it should be distinguished by a peculiar name; and, having consulted with several of the eminent and ingenious chemists of this country, I have been induced to give it the name of Columbium.

POSTSCRIPT.

It appears proper to mention some unsuccessful attempts, which I have lately made to reduce the white oxide.

Fifty grains were put into a crucible coated with charcoal; and, being covered with the same, the crucible was closely luted, and was exposed to a strong heat, in a small wind-furnace, during about one hour and an half. When the crucible was broken, the oxide was found in a pulverulent state; and, from white, was become perfectly black.

In order to form a phosphuret, some phosphoric acid was poured upon a portion of the white oxide; and, being evaporated

to dryness, the whole was put into a crucible coated with charcoal, as above described. The crucible was then placed in a forge belonging to Mr. CHENEVIX; and a strong heat was kept up for half an hour.

The inclosed matter was spongy, and of a dark brown; it in some measure resembled phosphuret of titanium.

After this, we wished to try the effect of a still greater heat; but in this experiment the crucible was melted.

The above experiments shew, that the white oxide, like several other metallic substances, may be deoxidated to a certain degree, without much difficulty, but that the complete reduction of it is still far from being easily effected.

IV. *A Description of the Anatomy of the* Ornithorhynchus paradoxus. *By* Everard Home, *Esq. F. R. S.*

Read December 17, 1801.

THE subjects from which the following description is taken, were sent from New South Wales, to Sir JOSEPH BANKS, who very obligingly submitted them to my examination.

These were two specimens preserved in spirit; one male, the other female. The male was rather larger than the female, and in every respect a much stronger animal; they had both arrived at their full growth, or nearly so, as the epiphyses were completely united to the bodies of the bones, which is not the case in growing animals.

The natural history of this animal is at present very little known. Governor HUNTER, who has lately returned from New South Wales, where he had opportunities of seeing them alive, has favoured me with the following particulars respecting them.

The Ornithorhynchus is only found in the fresh-water lakes, of which there are many in the interior parts of the country, some three quarters of a mile long, and several hundred yards broad. This animal does not swim upon the surface of the water, but comes up occasionally to breathe, which it does in the same manner as the turtle. The natives sit upon the banks, with small wooden spears, and watch them every time they come to the surface, till they get a proper opportunity of striking

them. This they do with much dexterity; and frequently succeed in catching them in this way.

Governor HUNTER saw a native watch one for above an hour before he attempted to spear it, which he did through the neck and fore leg: when on shore, it used its claws with so much force, that they were obliged to confine it between two pieces of board, while they were cutting off the barbs of the spear, to disengage it. When let loose, it ran upon the ground with as much activity as a land tortoise; which is faster than the structure of its fore feet would have led us to believe. It inhabits the banks of the lakes, and is supposed to feed in the muddy places which surround them; but the particular kind of food on which it subsists, is not known.

Description of the external Appearances.

The male is $17\frac{1}{2}$ inches in length, from the point of the bill to the extremity of the tail. The bill is $2\frac{1}{4}$ inches long; and the tail, measuring from the anus, $4\frac{1}{2}$ inches.

The body of the animal is compressed, and nearly of the same general thickness throughout, except at the shoulders, where it is rather smaller. The circumference of the body is 11 inches. There is no fat deposited between the skin and the muscles.

The female measures in length $16\frac{1}{4}$ inches, and in circumference 11 inches. The size of the body is rendered proportionally larger than that of the male, by a quantity of fat lying every where under the skin.

The male is of a very dark brown colour, on the back, legs, bill, and tail; the under surface of the neck and belly is of a silver gray. In the female, the colour of the belly is lighter.

The hair is made up of two kinds; a very fine thick fur, $\frac{1}{2}$ of an inch long, and a very uncommon kind of hair, $\frac{3}{4}$ of an inch long; the portion next the root has the common appearance of hair, but, for $\frac{1}{4}$ of an inch towards the point, it becomes flat, giving it some faint resemblance to very fine feathers: this portion has a gloss upon it; and, when the hair is dry, the different reflections from the edges and surfaces of these longer hairs, give the whole a very uncommon appearance. The fur and hair upon the belly, is longer than that upon the back.

Externally there is no appearance of the organs of generation, in either sex; the orifice of the anus being a common opening to the rectum and prepuce in the male, and to the rectum and vagina in the female.

There is no appearance, that could be detected, of nipples; although the skin on the belly of the female was examined with the utmost accuracy for that purpose.

The head is rather compressed. The bill, which projects beyond the mouth, in its appearance resembles that of the duck; but is in its structure more like that of the spoonbill, the middle part being composed of bone, as in that bird; it has a very strong cuticular covering.

In the upper portion of the bill, the lip extends for half an inch anteriorly, and laterally, beyond the bony part, and is thick and fleshy. The upper surface of the bill is uniformly smooth, and does not terminate where the hair begins, but is continued on for $\frac{3}{4}$ of an inch, forming a cuticular flap, which lies loose upon the hair. In the dried specimens that have been brought to Europe, the flap has been contracted in drying, and stands

up perpendicularly; this, however, is now ascertained not to be its natural situation.

The under surface of the upper half of the bill is also smooth; but has two hard ridges of a horny nature, an inch long and $\frac{1}{10}$ of an inch broad, situated longitudinally, one on each side of the middle line of the bill.

The lower portion of the bill is much smaller than the upper; and, when opposed to it, the lip of the upper extends beyond it for the whole of its breadth. The edges of the lip of this lower portion have deep serræ, in a transverse direction, like those in the duck's bill, but they are entirely confined to the fleshy lip; and, immediately within these serrated edges are grooves, lined with a horny substance, which receive, in the closed state of the bill, the ridges of the upper portion above described. There is also a cuticular flap extended upon the hair, as in the upper portion of the bill.

The nostrils are two orifices, very close to each other, near the end of the bill; the upper lip projecting $\frac{3}{4}$ of an inch beyond them.

The eyes are very small; they are situated more upon the upper part of the head than is usual, and are directly behind the loose edge of the cuticular flap belonging to the bill. The eyelids are circular orifices, concealed in the hair; and in the male are with difficulty discovered, but in the female there is a tuft of lighter hair, which marks their situation.

The external ears are two oval slits, directly behind the eyes, and much larger than the orifices of the eyelids.

The teeth, if they can be so called, are all grinders; they are four in number, situated in the posterior part of the mouth,

one on each side of the upper and under jaw, and have broad flattened crowns. In the smaller specimens before examined, each of these large teeth appeared to be made up of two smaller ones, distinct from each other. The animal, therefore, most probably sheds its teeth as it increases in size. They differ from common teeth very materially, having neither enamel nor bone, but being composed of a horny substance only embedded in the gum, to which they are connected by an irregular surface, in the place of fangs. When cut through, which is readily done by a knife, the internal structure is fibrous, like nail; the direction of the fibres is from the crown downwards.

This structure is similar to that of the horny crust which lines the gizzard in birds.

Between the cheek and the jaw, on each side of the mouth, there is a pouch, as in the monkey tribe, lined with a cuticle. When laid open, it is $1\frac{1}{2}$ inch long, and the same in breadth. In the female, it contained a concreted substance, the size of a very small nut, one in each pouch: this, when examined in the microscope, was made up of very small portions of broken crystals.

Besides these grinding teeth, there are two small pointed horny teeth upon the projecting part of the posterior portion of the tongue, the points of which are directed forwards, seemingly to prevent the food from being pushed into the fauces during the process of mastication. This circumstance, of small teeth on the tongue, is, I believe, peculiar to this animal, not being met with in other quadrupeds. In the tongue of the flamingo there is a row of short teeth on each side, but in no other bird that I have seen. The teeth are represented in the annexed drawing.

The fore legs are short, and the feet webbed; the length of

the leg and foot, to the end of the web, is about three inches. On each foot there are five toes, united together by the web, which is very broad, and is continued beyond the points of the toes, for nearly an inch. On each toe there is a rounded straight nail, which lies loose upon the membrane forming the web. The palms of the feet are covered with a strong cuticle; and there is a small prominence at the heel.

The hind legs are nearly of the same length as the fore legs, but stronger. Each leg has five toes, with curved claws; these are webbed, but the web does not extend beyond the points of the toes. The four outer toes are at equal distances from each other; but the inner one is at a much greater distance from the one next it. The under surface of the foot is defended by a strong cuticular covering.

In the male, just at the setting on of the heel, there is a strong crooked spur, $\frac{1}{2}$ an inch long, with a sharp point, which has a joint between it and the foot, and is capable of motion in two directions. When the point of it is brought close to the leg, the spur is almost completely concealed among the hair; when directed outwards, it projects considerably, and is very conspicuous. It is probably by means of these spurs or hooks, that the female is kept from withdrawing herself in the act of copulation; since they are very conveniently placed for laying hold of her body on that particular occasion. The female has no spur of this kind.

The tail, in its general shape, is very similar to that of the beaver. The hair upon its upper surface is long and strong; it has a coarse appearance. The under surface, if superficially examined, appears to have no hair; but, when more closely inspected, is found to be covered with short straggling hairs.

Description of the internal Parts.

The panniculus carnosus, which lies immediately under the skin, and extends over the greatest part of the body, is exceedingly strong.

The tongue is two inches long; it lies in the hollow between the two jaws, but does not project any way into the bill, being confined to its situation, except a very small portion at the tip. It is smallest at the point, and becomes larger towards the root; the posterior portion becomes very large, and rises considerably higher than the rest, forming a projection, on the anterior part of which are the two small teeth already mentioned. The tongue is covered with short cuticular papillæ, the points of which are directed backwards.

The velum pendulum of the palate is very broad. The glottis is uncommonly narrow; and the epiglottis proportionally small. The rings of the trachea are broad for their size; they do not meet behind, but nearly so. The tongue and epiglottis are represented in Plate II. Fig. 2.

In the structure of the bones of the chest, there are some peculiarities which deserve notice.

The ribs are sixteen in number: the six superior are united to the sternum, which is narrow and very moveable; the other ten terminate anteriorly in broad, flattened, oval, bony plates, which overlap each other in the contracted state of the chest, and are united together by a very elastic ligamentous substance, which admits of their being pulled to some distance; so that the capacity of the chest can undergo a very unusual degree of change.

The ribs are not connected to the sternum by their cartilages, as in other quadrupeds, but by bone; the cartilaginous portion

being only about an inch long, and situated at some distance from the sternum, between two portions of rib, forming a kind of joint at that part. There is no ensiform cartilage.

On the upper end of the sternum is a bone an inch long, which at its upper part has two processes that answer the purpose of clavicles, and unite with the upper part of the scapulæ, keeping them at a proper distance. The scapulæ have a very unusual shape: the posterior part is more like the imperfect scapula in the bird; and the flat part is situated with one edge under the bone, immediately above the sternum. The other edge forms the glenoid cavity, for the articulation of the os humeri; so that the fore legs have their connection with the trunk more forward than in other quadrupeds; and the scapula itself is much more firmly confined to its situation.

This bone above the sternum, with the anterior part of the two scapulæ, forms a bony covering of some strength, under which pass the great blood-vessels of the neck, secured from compression.

The appearance of the ribs, sternum, and other bones, is represented in Plate III.

The heart is situated in the middle line of the chest, its apex pointing to the sternum, and is inclosed in a strong pericardium: it is made up of two auricles and two ventricles. The foramen ovale between the auricles was closed, nor was there any communication between the ventricles. The right auricle is very large, and has two ascending venæ cavæ; that to the left winding round the basis of the heart, and forming the subclavian and jugular vein of that side, after giving off the vena azygos. This is similar to the kangaroo, beaver, otter, and many other animals. The aorta and other arteries are small.

The lungs are large in size, corresponding to the capacity of the chest. On the right side there are two lobes; there is a small azygos lobe under the heart; and in the left side only one. Instead of a portion of the lungs being above the heart, as in other animals, the heart may be said to be above the lungs; for they only embrace its sides, and do not surround its upper surface, but extend downwards, into the more moveable part of the cavity of the chest.

The diaphragm is very broad, and every where towards the circumference is muscular, having only a small central portion, which is tendinous, immediately under the heart.

The œsophagus is extremely small, more particularly at its origin behind the larynx, where the fauces terminate in it.

The stomach is a membranous bag, of an oval form, into which the œsophagus can hardly be said to enter, being rather continued along one end of the oval, till it forms the duodenum; so that the stomach appears to be a lateral dilatation of a canal, which is the œsophagus where the dilatation is formed, and becomes the duodenum immediately afterwards, at which part the coats are thickened, forming the valve of the pylorus.

The stomach is smaller than in most other animals; in this respect it is like the true stomach of birds. In the collapsed state it is only $1\frac{1}{2}$ inch long, and $\frac{3}{4}$ of an inch broad. This is exactly double the size of one of the pouches in the cheek.

The duodenum makes a turn in the right side of the abdomen; then crosses the spine, and becomes a loose intestine. The small intestines are strung upon a loose, broad, transparent mesentery. The origin of the colon is only to be distinguished by a small lateral appendage, $1\frac{1}{2}$ inch long, and $\frac{1}{4}$ of an inch in diameter, going off from the side of the intestine, which is not

altered in its size at this part. This process corresponds to the cæcum: it is unlike the cæcum in quadrupeds, but resembles that in birds, only is much smaller, and in general they have two; but the bittern and heron have only one. From this part, the colon passes up the left side, fixed to its situation by being attached to the omentum; then goes across the body, and becomes rectum, which gradually increases in size, and is very capacious before it terminates at the anus.

The small intestines are four feet four inches long. The colon and rectum are one foot four inches long.

The rectum opens externally at the root of the tail, $1\frac{1}{2}$ inch below the pelvis. On each side of the anus is a large solid body, about the size of the testicle, which proves to be a gland, whose ducts open by several orifices into the rectum. In the female, the same glands are met with, but of a much smaller size.

The mesentery is free from fat; nor are there any fatty appendages, or longitudinal bands, on the colon. The mesenteric glands are of the size of millet-seeds; they are numerous, and scattered over the mesentery. The lacteals are small.

The internal surface of the stomach is uniformly smooth. The duodenum has valvulæ conniventes, which are transverse: these are not met with in the jejunum and ilium; but in them the internal membrane is studded over with glands. There is no appearance whatever of valve at the beginning of the colon; but there are ten dotted lines, which run in a longitudinal direction, at equal distances from one another, and have their origin at the orifice of the cæcum: these dots, upon a close inspection, prove to be the projecting orifices of ducts belonging to the glands of the intestine. The cavity of the small cæcum is very cellular, as is shown in Plate II. Fig. 3.

The omentum is a thin transparent membrane, without any fat in it, originating from the side of the stomach next the duodenum, and also from that intestine anteriorly: on the left side it hangs loose, and the spleen is connected to it; but, on the right, after it reaches the colon, it surrounds that gut, and returns to the spine; so that, although the colon is confined by the omentum, there is no part of that membranous bag projecting beyond it.

The liver is composed of four lobes, besides the small lobe or lobulus Spigelii. The gall-bladder is in the usual situation, and of the common size. The cystic and hepatic ducts unite into one, and are joined by the pancreatic duct before their termination in the duodenum, which is about an inch from the pylorus.

The pancreas is spread upon the great and little omentum, as in the sea-otter, and is made up of small parts, in a very similar manner.

The spleen consists of two very long slender bodies, united together at one end for the length of half an inch: one of these portions is six inches, the other four inches long.

The kidnies are conglobate, and lie in the usual situation. The capsulæ renales are rather small. The ureters are pellucid and small.

The urinary bladder is not situated in the pelvis, but just above it, in the cavity of the abdomen, and is attached to the peritonæum lining the abdominal muscles.

The skull is rather flattened upon the upper surface: its cavity is capacious; and there is a bony process projecting from the cranium, in the place of the falx of the dura mater. This, I believe, is not the case in any other quadruped: it is met with

in some birds in a less degree, as in the parrot and the spoon-bill; which last bird, in the structure of its beak, bears some analogy to this animal. The tentorium is entirely membranous.

The brain was not in a state to admit of its structure being accurately examined; but it appears to be made up of the same parts as those of quadrupeds in general.

The olfactory nerves are small, and so are the optic nerves; but the fifth pair, which supplies the muscles of the face, are uncommonly large. We should be led, from this circumstance, to believe that the sensibility of the different parts of the bill is very great, and therefore that it answers the purpose of a hand, and is capable of nice discrimination in its feeling.*

The eye is very small, and is nearly spherical: the globe is about $\frac{1}{4}$ of an inch in diameter; the cornea $\frac{3}{16}$ of an inch in diameter. There is a membrana nictitans; and the eyelid is very loose upon the eyeball; it is probably capable of great dilatation and contraction.

The organ of smell, in its construction resembles that of other quadrupeds, and may be said to consist of two turbinated bones in each nostril; that next the bill is the largest, and has the long axis in the direction of the nostril; its external surface is very irregular. The posterior one is shorter, projects further into the nostril, and is situated transversely, with respect to the nostril. As the external openings of the nose are at the end of the bill, there is a canal of an unusual length for the air to pass through, before it is applied to the immediate organ, unless there is an extension of the branches of the olfactory nerve upon the linings of the cavity, so as to make it a part of it. The external

* The same observations were made by Professor BLUMENBACH, of Gottingen, who first dissected these nerves.

opening of the ear is at a great distance from the organ; and there is a cartilaginous canal, the size of a crow-quill, winding round the side of the head, upon the outside of the temporal muscle, leading to the orifice in the temporal bone.

The membrana tympani is larger than in other quadrupeds of the same size: it is of an oval form; and the central part is drawn in, making its external surface concave. It has only two bones; one passing directly from the membrane towards the foramen ovale, upon which there is a second bone, imperfectly resembling the stapes, having a flat surface of a circular form upon the orifice, and a small neck, by which it is united to the other bone.

This structure of the bones is less perfect or complex than in other quadrupeds; so that the organ altogether bears a greater resemblance to that of the bird.

The organs of generation in this animal have several peculiarities of a very extraordinary nature.

The male organs do not appear externally; so that the distinguishing mark of the sex is the spur on the hind leg.

The testicles are situated in the cavity of the abdomen, immediately below the kidneys: they are large for the size of the animal. The epididymis is connected to the body of the testicle by a broad membrane, which admits of its lying very loose.

The penis in this animal does not, as in other quadrupeds, give passage to the urine. It is entirely appropriated to the purpose of conveying the semen; and a distinct canal conducts the urine into the rectum, by an opening about an inch from the external orifice of the intestine. The gut, at this part, is defended from the acrimony of the urine, by the mucus secreted by two glands already described, which probably for this reason

are very large in the male, but small in the female. The opening of the meatus urinarius, and the orifices of the glands, are represented in Plate IV.

The penis is short and small in its relaxed state; and its body does not appear capable of being very much enlarged when erected. The prepuce is a fold of the internal membrane of the verge of the anus, as in the bird; and the penis, when retracted, is entirely concealed.

The glans penis is double; one glans having its extremity directed to the right, the other to the left; and, as they supply two distinct cavities with semen, they may be considered as two penises. This is an approach to the bird, many of which have two. Each glans has, at its extremity, pointed conical papillæ, surrounding a central depression. In one glans, the papillæ are five in number, in the other four. When the urethra is laid open from the bladder into the rectum, about half an inch from its termination it communicates with the proper urethra of the penis, which afterwards divides into two, one going to each glans, in the centre of which is a cavity communicating directly with the papillæ, the points of which are pervious, forming the orifices by which the semen is evacuated.

The vasa deferentia open into the membranous part of the urethra, before it comes to the root of the penis.

Not being aware of so extraordinary a structure, and the parts not being in a perfect state of preservation, they were too much injured by dissection before it was discovered, to admit of their being prepared by injection. The appearance of these parts is delineated in Plate IV.

There was no appearance of vesiculæ seminales.

The female organs open into the rectum, as in the bird. Just

within the anus there is a valvular projection, between the rectum and vagina, which appears to be the proper termination of the rectum. This also is similar to the bird.

There was no appearance of clitoris, that could be observed.

The vagina is $1\frac{1}{2}$ inch long: its internal membrane is rugous; the rugæ being in a longitudinal direction. At the end of the vagina, instead of an os tincæ, as in other quadrupeds, is the meatus urinarius; on each side of which is an opening leading into a cavity, resembling the horn of the uterus in the quadruped, only thinner in its coats. Each of these cavities terminates in a fallopian tube, which opens into the capsule of an ovarium.

The ovaria are very small: they were not in a very perfect state of preservation, but bore a general resemblance to those of other quadrupeds.

This structure of the female organs is unlike any thing hitherto met with in quadrupeds; since, in all of them that I have examined, there is the body of the uterus, from which the horns go off, as appendages. The opossum differs from all other animals in the structure of these parts, but has a perfectly formed uterus; nor can I suppose it wanting in any of the class Mammalia.

This animal having no nipples, and no regularly formed uterus, led me to examine the female organs in birds, to see if there was any analogy between the oviducts in any of that class, and the two membranous uteri of this animal; but none could be observed; nor would it be easy to explain how an egg could lie in the vagina, to receive its shell, as the urine from the bladder must pass directly over it. Finding they had no resemblance to the oviducts in birds, I was led to compare them with the uteri of those lizards which form an egg, that is afterwards deposited in

a cavity corresponding to the uterus of other animals, where it is hatched; which lizards may therefore be called ovi-viviparous; and I find a very close resemblance between them. In these lizards there are two uteri, that open into one common canal or vagina, which is extremely short; and the meatus urinarius is situated between these openings. The coats of these uteri are thinner than those of the uteri of quadrupeds of the same size.

In the ovi-viviparous dog-fish, the internal organs of the female have a very similar structure. There is therefore every reason to believe, that this animal also is ovi-viviparous in its mode of generation.

EXPLANATION OF THE DRAWINGS.

See PLATES II. III. and IV.

PLATE II.

Fig. 1. Represents the hind leg of the male, in order to shew the situation and appearance of the spur.

Fig. 2. Represents the tongue, in its natural situation; and shows its relative position to the grinding teeth, and the lower portion of the bill; also the two pointed teeth upon the tongue itself.

On the outside of the jaw, on each side, are the pouches for the food.

The glottis, epiglottis, and œsophagus, are represented of the natural size.

Fig. 3. The loculated cæcum, with a portion of the ilium and colon.

PLATE III.

Represents the bones of the chest and pelvis, in their relative

situation, to show the uncommon shape of the scapulæ, which are not connected with the chest, but with a bone placed above the sternum, the upper part of which answers the purpose of clavicles; the anterior part of each scapula passes under this bone laterally, forming with it a bony case for this part of the neck.

Another peculiarity is, the cartilages of the ribs not being placed next the sternum, but between two portions of the rib. The false ribs have their cartilages terminated by thin bony scales, which slide on one another in the motions of the chest.

The pelvis is unusually small, and has the two moveable bones, attached to the os pubis, which are met with in the kangaroo.

aaa. The bone which corresponds to the clavicles in other animals.

bbb. The left scapula.

ccc. The bony scales along the margin of the chest.

ddd. The cartilages of the true ribs.

ee. The moveable bones of the pelvis.

Plate IV.

Fig. 1. Represents the penis in a relaxed state, but drawn out to its full extent, with its relative situation to the rectum and testicles, which are contained in the cavity of the abdomen.

aa. The bodies of the testicles.

bb. The epididymis.

c. The urinary bladder.

dd. The rectum.

ee. Two glands, whose ducts enter the rectum by a number of small orifices.

f. The body of the penis, whose external covering is a continuation of the lining of the lower part of the rectum.

gg. The double glans: at the point of the right one are five conical papillæ, and at the point of the left only four, which are open at their extremities; through these orifices the semen passes.

b. The opening of the urethra into the rectum.

Fig. 2. A view of the uteri and vagina.

aa. The vestibulum, common to the rectum and vagina.

bb. The cut edges of the rectum; the gut being dissected off to expose the vagina.

c. The vagina.

d. The meatus urinarius.

e. The bladder.

ff. The orifices leading to the uteri.

gg. The two uteri.

bb. The fallopian tubes.

ii. The ovaria, enclosed in the capsules.

Fig. 1.

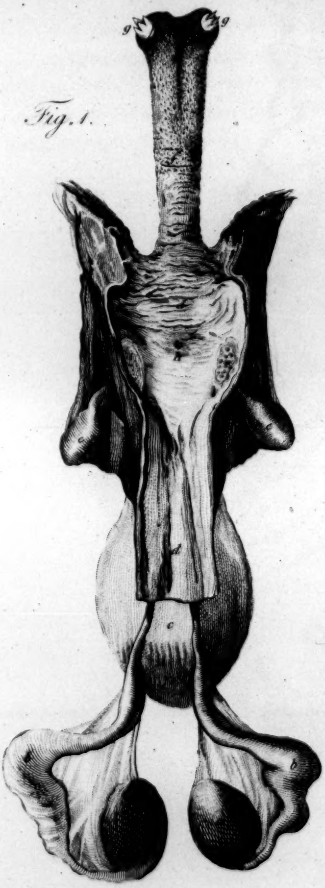
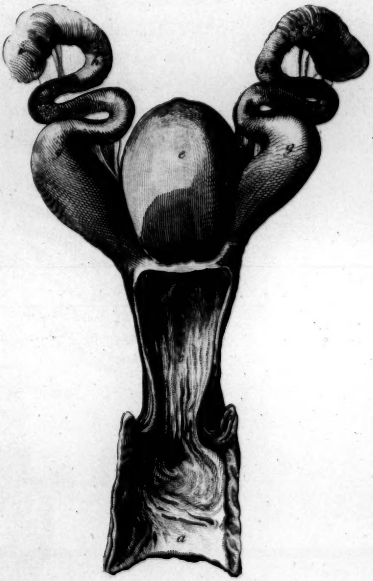


Fig. 2.



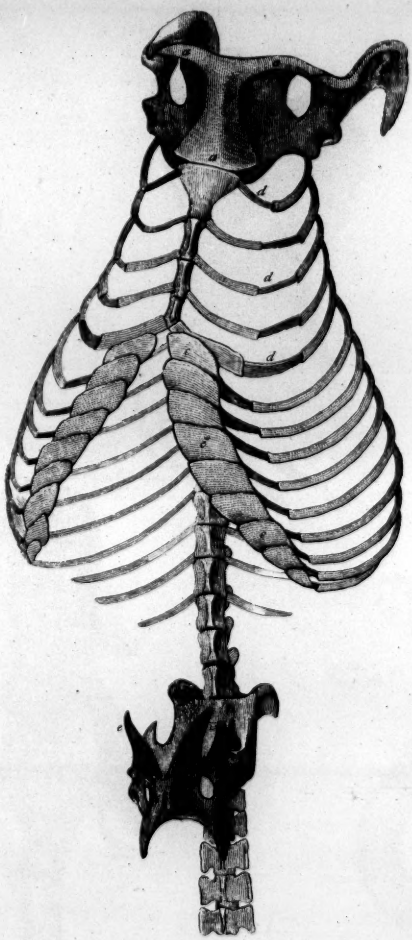


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Fig. 1.

Fig. 2.

Fig. 3.

V. *On the Independence of the analytical and geometrical Methods of Investigation; and on the Advantages to be derived from their Separation.* *By* Robert Woodhouse, *A. M. Fellow of Caius College*, Cambridge. *Communicated by* Joseph Planta, *Esq. Sec. R. S.*

Read January 14, 1802.

ONE of the objects of the paper which last year I had the honour of presenting to the Royal Society, was to shew the insufficiency in mathematical reasoning, of a principle of analogy, by which the properties demonstrated for one figure were to be transferred to another, to which the former was supposed to bear a resemblance; and the argument for the insufficiency of the principle was this, that the analogy between the two figures was neither antecedent to calculation, nor independent of it, and consequently could not regulate it; that analogy was the object of investigation, not the guide; the result of demonstration, not its directing principle.

Having shewn that analogy could not establish the truth of certain mathematical conclusions, I next endeavoured to shew why such conclusions had been rightly inferred; not by proposing any new excogitated principle, nor by pointing out an hitherto unobserved intellectual process; but I conceived they might be obtained by operations conducted in a manner similar to that by which all reasoning with general terms is conducted,

and that the relations between the symbols or general terms were to be established by giving the true meaning to the connecting signs, which indicate not so much the arithmetical computation of quantities, as certain algebraical operations. It was further observed, that, from certain established formulas, abridged symbols or general terms might be formed, which consequently must have their signification dependent on such formulas; and that, although the parts of certain abridged expressions could not separately be arithmetically computed, yet the expressions themselves might be legitimately employed in all algebraic operations.

The chief object of my paper was to shew, that operations with imaginary quantities, as they are called, were strictly and logically conducted, that is, conducted after the same manner as operations with quantities that can be arithmetically computed: the question, whether calculation with imaginary symbols is commodious or not, was then slightly discussed. I have since attentively considered it, and, what usually happens in such cases, my inquiries have been extended beyond their original object; for, actual research has convinced me of what there were antecedent reasons for suspecting, that not only in the theory of angular functions, demonstration is most easy and direct by giving to quantities their true and natural* representation; but, that the introduction of expressions and formulas not analytical, into analytical investigation, has caused much ambiguity, confused notion, and paradox; that it has made

* $(2^{-1}).\left\{{}_{\cdot}{}^{x\sqrt{-1}}+{}_{\cdot}{}^{-x\sqrt{-1}}\right\}$, $(2\sqrt{-1})^{-1}.\left\{{}_{\cdot}{}^{x\sqrt{-1}}-{}_{\cdot}{}^{-x\sqrt{-1}}\right\}$ &c. I call the *natural* representations of the cosines, sines, &c. of an arc x; because, admitting the algebraical notation, they, by strict inference, adequately, unambiguously, and solely, represent the cosines, sines, &c.

demonstration prolix, by rendering it less direct, and has made it deficient in precision and exactness, by diverting the mind from the true source and derivation of analytical expression.

The expressions and formulas alluded to are geometrical, that is, taken from the language of geometry, and established by its rules: they are to be found mixed with analytical* expressions and reasonings, in all works on abstract science; and, as they are certainly foreign and circumlocutory, if it can be shewn that they are not essentially necessary, there will exist an argument for their exclusion, especially if it appears that in analytical investigation they are productive of the evils above mentioned.

That, in algebraical calculation, geometrical expressions and formulas are not essentially necessary, perhaps this short and easy consideration may convince us; since algebra is an universal language, it ought surely to be competent to express the conditions belonging to any subject of inquiry; and, if adequate expressions be obtained, then there is no doubt that with such, reasoning or deduction may be carried on.

All expressions and formulas, such as, sin. x, cos. x, hyp. log. x, sin. $nx = 2$ cos. x. sin. $(n-1)\,x -$ sin. $(n-2)\,x$,

* The terms analysis, analytical, algebra, algebraical, have been so often distinguished, and so often confounded, that I shall not take the trouble again to distinguish them. I use the words analytical, algebraical, indifferently, in contradistinction to geometrical. The first relates to an arbitrary system of characters; the latter to a system of signs, that are supposed to bear a resemblance to the things signified, and in which system, lines and diagrams are used as the representatives of quantity: and I am principally induced to use the words indifferently, because, if analytical were properly defined, another word with a sufficient extent of meaning could not be found; for, by an improper limitation, the word algebraical has not an extensive signification, being frequently used in contradistinction to transcendental, exponential, &c.

$\int x \cdot (1 - x^2)^{\frac{1}{2}} =$ circular arc, $\int x \cdot \sqrt{\frac{1 + n^2 x^2}{1 - x^2}} =$ elliptical arc, &c. are geometrical, or involve geometrical language: they suppose the existence of a particular system of signs, and method of deduction; and relate to certain theorems, established conformably to such system and method.

I. Sin. x, cos. x, tang. x, &c. These expressions are borrowed from geometry; but, analytically, denote certain functions of x. Typographically considered, these expressions are more commodious than $(2\sqrt{-1})^{-1}\left\{\varepsilon^{x\sqrt{-1}} - \varepsilon^{-x\sqrt{-1}}\right\}$, $(2)^{-1}\left\{\varepsilon^{x\sqrt{-1}} + \varepsilon^{-x\sqrt{-1}}\right\}$, &c. but this is the sole advantage; for, all analytical operations with these latter signs are much easier, and more expeditious, than with the former; since they are carried on after a manner analogous to that by which operations with similar expressions are. But, if the geometrical expressions be retained, then, in order to calculate with them, recourse must be had to the geometrical method, proceeding by the similarity of triangles, the doctrines of proportions, and of prime and ultimate ratios; so that, in the same investigation, two methods of deduction, between which there is no similarity, must be employed.

II. The value of $\int x \cdot (1 + x)^{-1}$, is said to be a portion of the area of an hyperbola intercepted between two ordinates to its assymptotes; but this is a foreign and circumlocutory mode of expression; since, to find the value of the area, $x \cdot (1 + x)^{-1}$ must be expanded, and the integrals of the several terms taken; and this same operation must have taken place, in order to approximate to the value of $\int x \cdot (1 + x)^{-1}$, if no such curve as the hyperbola had ever been invented.

III. $\int x \cdot \left\{1 - x^2\right\}^{-\frac{1}{2}}$ is said to equal the arc of the circle

rads. 1, sin. x; but nothing is gained by this; since, in order to find the arc of a circle, $x \cdot (1-x^2)^{-\frac{1}{2}}$ is expanded, and the integrals of the several parts taken and added together. To shew (if it is necessary to add any thing more on so clear a point) that $\int x \cdot \left\{1-x^2\right\}^{-\frac{1}{2}} =$ arc circle, is merely a mode of expression borrowed from geometry; suppose the investigation of the properties of motion to have been prior to the investigation of the properties of extension, for, that the science of geometry was first invented is properly an accidental circumstance, then, such an expression as $\int x \cdot \left\{1-x^2\right\}^{-\frac{1}{2}}$ might have occurred, and its value must have been exhibited as it *really* is now, that is, by expanding it, and integrating the several terms.

IV. It is an objection certainly against these modes of expression, that they are foreign, and tend to produce confused and erroneous notions; for the student may be led by them to believe, that the determination of the values of certain analytical expressions, essentially require the existence of certain curves, and the investigation of their properties. But there is a more valid objection against them, which is, that they divert the mind from the true derivation of such expressions as $\frac{x^2}{2}$, $x \cdot (1-x^2)^{-\frac{1}{2}}$, &c. and consequently tend to produce ambiguity and indirect methods; for although, in order to obtain approximately the numerical value of $\int \frac{x^2}{2}$, $\int x \cdot (1-x^2)^{-\frac{1}{2}}$, &c. it is convenient to expand the expressions, and to take the integrals of the resulting terms, yet, if the symbol $\int$ denotes a reverse operation, $\int \frac{x^2}{2}$, $\int x \cdot (1-x^2)^{-\frac{1}{2}}$ are not properly and by strict inference equal to $(x-1) - \frac{1}{2}\left\{x-1\right\}^2 + \frac{1}{3}\left\{x-1\right\}^3 -$, &c. and $x + \frac{x^3}{2.3} + \frac{3x^5}{5.8} +$, &c. But, in order to explain clearly what I mean, it is

necessary to state what I understand by the integral or fluent of an expression.

V. Let φx denote a function of x; if x be increased by o, then φx becomes $\phi(x+o)$, and $\phi(x+o)$, developed according to the powers of o, becomes $\varphi x + \mathrm{P}o + \frac{\mathrm{Q}}{1.2}o^2 + \frac{\mathrm{R}}{1.2.3}o^3$, &c. where P is derived from φx, Q from P, R from Q, &c. by the same law; so that the manner of deriving P being known, Q, R, &c. are known. The entire difference or increment of φx is $\phi(x+o) - \varphi x$; the differential or fluxion of φx is only a part of the difference or $\mathrm{P}.o$. If, instead of o, dx, or $x^{\cdot}$, be put, it is $\mathrm{P}.dx$ or $\mathrm{P}x^{\cdot}$; the integral or fluent of $\mathrm{P}x^{\cdot}$ is that function from which $\mathrm{P}x^{\cdot}$ is derived; and, in order to remount to it, we must observe the manner or the operation by which it was deduced; and, by reversing such operation, the integral or fluent is obtained. Now, in taking the fluxion of certain functions of x, it appears there are conditions to which the indices of x without and under the vinculum are subject: hence, whether or not a proposed fluxion can have its fluent assigned, we must see if the fluxion has the necessary conditions. Expressions such as $\frac{x^{\cdot}}{x}$, $\frac{x^{\cdot}}{1+x}$, $\frac{x^{\cdot}}{\sqrt{1-x^2}}$, &c. have not these conditions; and consequently there is no function φx of x, such that the second term of the developement of $\phi(x+x^{\cdot})$ is equal either to $\frac{x^{\cdot}}{x}$, or $\frac{x^{\cdot}}{1+x}$, or $\frac{x^{\cdot}}{\sqrt{1-x^2}}$, or, &c. There are, however, integral equations from which such expressions may be derived. Thus, let $x = \varepsilon^{z}$, then $\frac{x^{\cdot}}{x} = z^{\cdot}$, let $1+x = \varepsilon^{z}$ $\therefore$ $\frac{x^{\cdot}}{1+x} = z^{\cdot}$, let $x = \frac{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}}{2\sqrt{-1}}$ $\therefore$ $\frac{x^{\cdot}}{\sqrt{1-x^2}} = z^{\cdot}$.

Now, from these equations, the differential equations $\frac{x^{\cdot}}{x} = z^{\cdot}$, $\frac{x^{\cdot}}{1+x} = z^{\cdot}$, $\frac{x^{\cdot}}{\sqrt{1-x^2}}$, &c. may, by expunging the exponential

quantities, be derived; consequently, if the symbol $\int$ is to designate a reverse operation, I can only know what that reverse operation is, by attending to the manner by which the expressions affected with the symbol $\int$ were derived. Hence,

VI. $\int \frac{x}{x} = z$ when $x = \epsilon^z$.

$\int \frac{x}{1+x} = z$ when $1 + x = \epsilon^z$.

$\int \frac{x}{\sqrt{1-x^2}} = z$ when $x = (2\sqrt{-1})^{-1}\left\{\epsilon^{z\sqrt{-1}} - \epsilon^{-z\sqrt{-1}}\right\}$.

In like manner,

$\int x \cdot (1+x^2)^{-\frac{1}{2}} = z$, $x + \sqrt{1+x^2} = \epsilon^z$ or $x = \frac{\epsilon^z - \epsilon^{-z}}{2}$.*

$\int x \cdot (2x+x^2)^{-\frac{1}{2}} = z$, $1 + x + \sqrt{2x+x^2} = \epsilon^z$.

$\int \frac{2x}{1-x^2} = z$, $\frac{1+x}{1-x} = \epsilon^z$ or $x = \frac{\epsilon^z - 1}{\epsilon^z + 1}$.

$\int \frac{2x}{x\sqrt{1+x^2}} = z$, $\frac{\sqrt{1+x^2}-1}{\sqrt{1+x^2}+1} = \epsilon^z$ or $\sqrt{1+x^2} = \frac{1+\epsilon^z}{1-\epsilon^z}$,

or $x = \frac{2}{\epsilon^{\frac{z}{2}} - \epsilon^{-\frac{z}{2}}}$.

Again, suppose

$x = \left\{2\sqrt{-1}\right\}^{-1}\left\{\epsilon^{z\sqrt{-1}} - \epsilon^{-z\sqrt{-1}}\right\}$ $\therefore x' = 2^{-1}z' \cdot \left\{\epsilon^{z\sqrt{-1}} + \epsilon^{-z\sqrt{-1}}\right\}$,

but $\sqrt{1-x^2} = 2^{-1}\cdot\left\{\epsilon^{z\sqrt{-1}} + \epsilon^{-z\sqrt{-1}}\right\}$; consequently $x' = z'\sqrt{1-x^2}$,

or $z' = \frac{x'}{\sqrt{1-x^2}}$: hence, reversely,

$\int \frac{x'}{\sqrt{1-x^2}} = z$, x being $= (2\sqrt{-1})^{-1} \cdot \left\{\epsilon^{z\sqrt{-1}} - \epsilon^{-z\sqrt{-1}}\right\}$.

In like manner,

$\int \frac{-x'}{\sqrt{1-x^2}} = z$, $x = 2^{-1} \cdot \left\{\epsilon^{z\sqrt{-1}} + \epsilon^{-z\sqrt{-1}}\right\}$.

$\int \frac{x'}{\sqrt{2x-x^2}} = z$, $x = (1-2^{-1} \cdot \left\{\epsilon^{z\sqrt{-1}} + \epsilon^{-z\sqrt{-1}}\right\})$.

* I take no notice, at present, of the arbitrary quantities which may be introduced in the integration of these equations.

$$\int \frac{x^{\cdot}}{1-x^2} = z,\ x = \frac{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}}{\sqrt{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}},\ \text{or}\ \frac{\varepsilon^{z\sqrt{-1}} - 1}{\sqrt{-1}\,(\varepsilon^{z\sqrt{-1}} + 1)}.$$

$$\int \frac{x^{\cdot}}{x\sqrt{x^2-1}} = z,\quad x = \frac{2}{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}}.$$

And a variety of forms may be obtained, by substituting for x different functions of x, in the expression $\frac{x^{\cdot}}{\sqrt{1-x^2}}$. Hence, if the symbol $\int$ is made to denote a reverse operation, the integral equations of the preceding differential equations have been rightly assigned. All other methods of assigning the integrals, by the properties of logarithms, by circular arcs, by logarithmic and hyperbolic curves,* are indirect, foreign, and ambiguous.

VII. An instance or two will shew the advantage of adhering to the true and natural derivation of analytical expressions. Let x and y be the co-ordinates of a circle; then, $1 = x^2 + y^2$, and $y = \sqrt{(1-x^2)}$, now (arc)$^{\cdot}$ or $z^{\cdot} = \sqrt{(x^{\cdot 2} + y^{\cdot 2})} =$, in this instance, $x^{\cdot}\,(1-x^2)^{-\frac{1}{2}}$: but it has appeared, that if $x = \left\{2\sqrt{-1}\right\}^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\}$, $z^{\cdot} = x^{\cdot}\,(1-x^2)^{-\frac{1}{2}}$; consequently, in a circle, the co-ordinate x, or, in the language of trigonometry, the sine $x =$ developement of

$$(2\sqrt{-1})^{-1}.\ \left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\} = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} - \&c.$$

and y or cosine $= 2^{-1}.\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\} = 1 - \frac{z^2}{1.2} + \frac{z^4}{1.2.3.4} - \&c.$

1. This method of determining the series for the sine in terms of the arc, is, I think, simple, direct, and exact; it requires no assumption of a series with indeterminate coefficients, nor

* By the strange way of determining the meaning and value of analytical expressions from geometrical considerations, it should seem, as if certain curves were believed to have an existence independent of arbitrary appointment.

any preparatory process to shew that the value of the first coefficient must = 1.*

VIII. EULER demonstrated this formula to be true, viz. $\frac{\text{Arc}}{2} = \text{sin. arc} - \frac{1}{2}\text{ sin. 2arc} + \frac{1}{3}\text{ sin. 3arc} - \frac{1}{4}\text{ sin. 4arc} + \&c.$ The following is its analytical deduction,

$$x \cdot = x \cdot \left\{ \frac{\varepsilon^{z\sqrt{-1}} + 1}{\varepsilon^{z\sqrt{-1}} + 1} \right\} = x \cdot \left\{ \frac{\varepsilon^{z\sqrt{-1}}}{\varepsilon^{z\sqrt{-1}} + 1} \right\} + x \cdot \left\{ \frac{1}{\varepsilon^{z\sqrt{-1}} + 1} \right\}$$

$$= x \cdot \left\{ \frac{\varepsilon^{z\sqrt{-1}}}{\varepsilon^{z\sqrt{-1}} + 1} \right\} + x \cdot \left\{ \frac{\varepsilon^{-z\sqrt{-1}}}{\varepsilon^{-z\sqrt{-1}} + 1} \right\}$$

$$= x \cdot \left\{ \begin{array}{l} \varepsilon^{z\sqrt{-1}} - \varepsilon^{2z\sqrt{-1}} + \varepsilon^{3z\sqrt{-1}} - \&c. \\ + \varepsilon^{-z\sqrt{-1}} - \varepsilon^{-2z\sqrt{-1}} + \varepsilon^{-3z\sqrt{-1}} - \&c. \end{array} \right\}$$

$$\therefore x = \frac{1}{\sqrt{-1}} \left\{ \begin{array}{l} \varepsilon^{z\sqrt{-1}} - \frac{\varepsilon^{2z\sqrt{-1}}}{2} + \frac{\varepsilon^{3z\sqrt{-1}}}{3} - \&c. \\ - \varepsilon^{-z\sqrt{-1}} + \frac{\varepsilon^{-2z\sqrt{-1}}}{2} - \frac{\varepsilon^{-3z\sqrt{-1}}}{3} + \&c. \end{array} \right\}$$

and $\frac{x}{2} = (2\sqrt{-1})^{-1} \cdot \left\{ \varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}} \right\} - \frac{1}{2}(2\sqrt{-1})^{-1} \cdot \left\{ \varepsilon^{2z\sqrt{-1}} - \varepsilon^{-2z\sqrt{-1}} \right\} + \frac{1}{3}(2\sqrt{-1})^{-1} \left\{ \varepsilon^{3z\sqrt{-1}} - \varepsilon^{-3z\sqrt{-1}} \right\} - \&c.$

which is the analytical translation of EULER's formula.

IX. EULER likewise shewed that

$$\text{sin. } x = 2^n \cdot \cos.\frac{x}{2} \cdot \cos.\frac{x}{4} \cdot \cos.\frac{x}{8} \cdot \ldots\ldots \cos.\frac{x}{2^n} \cdot \text{sin.}\frac{x}{2^n}.$$

Which may be thus demonstrated,

$$\text{sin. } x = (2\sqrt{-1})^{-1} \left\{ \varepsilon^{x\sqrt{-1}} - \varepsilon^{-x\sqrt{-1}} \right\};$$

$$\text{but } (2\sqrt{-1})^{-1} \left\{ \varepsilon^{x\sqrt{-1}} - \varepsilon^{-x\sqrt{-1}} \right\} = 2 \cdot 2^{-1} \left\{ \varepsilon^{\frac{x}{2}\sqrt{-1}} + \varepsilon^{-\frac{x}{2}\sqrt{-1}} \right\},$$

$$\left\{ (2\sqrt{-1})^{-1} \varepsilon^{\frac{x}{2}\sqrt{-1}} - \varepsilon^{-\frac{x}{2}\sqrt{-1}} \right\}$$

$$= 2 \cdot 2^{-1} \left(\varepsilon^{\frac{x}{2}\sqrt{-1}} + \varepsilon^{-\frac{x}{2}\sqrt{-1}} \right) \cdot 2 \cdot 2^{-1} \left(\varepsilon^{\frac{x}{4}\sqrt{-1}} + \varepsilon^{-\frac{x}{4}\sqrt{-1}} \right) \cdot$$

$$(2\sqrt{-1})^{-1} \cdot \left\{ \varepsilon^{\frac{x}{4}\sqrt{-1}} - \varepsilon^{-\frac{x}{4}\sqrt{-1}} \right\}$$

* See LAGRANGE, Fonctions Analytiques, p. 26, LACROIX, Traité du Calcul. differentiel, &c. p. 56. LE SEUR, Sur le Calcul. diff. p. 105. EULER, Anal. Inf. Art. 133, 134.

$= 2 \cdot 2^{-1}\left(\varepsilon^{\frac{x}{2}\sqrt{-1}} + \varepsilon^{-\frac{x}{2}\sqrt{-1}}\right) \cdot 2 \cdot 2^{-1}\left(\varepsilon^{\frac{y}{2}\sqrt{-1}} + \varepsilon^{-\frac{y}{2}\sqrt{-1}}\right)$

$\cdot 2 \cdot 2^{-1}\left\{\varepsilon^{\frac{z}{2}\sqrt{-1}} + \varepsilon^{-\frac{z}{2}\sqrt{-1}}\right\}\left\{2\sqrt{-1}\right\}^{-1}\left(\varepsilon^{\frac{x}{2}\sqrt{-1}} - \varepsilon^{-\frac{x}{2}\sqrt{-1}}\right)$,

or, generally,

$= 2^{n} \cdot 2^{-1}\left\{\varepsilon^{\frac{x}{2}\sqrt{-1}} + \varepsilon^{-\frac{x}{2}\sqrt{-1}}\right\} \cdot 2^{-1}\left\{\varepsilon^{\frac{y}{2}\sqrt{-1}} + \varepsilon^{-\frac{y}{2}\sqrt{-1}}\right\} \cdot 2^{-1}$

$\left(\varepsilon^{\frac{z}{2}\sqrt{-1}} + \varepsilon^{-\frac{z}{2}\sqrt{-1}}\right\} \ldots .$

$\ldots 2^{-1}\left\{\varepsilon^{\frac{x}{2^n}\sqrt{-1}} + \varepsilon^{-\frac{x}{2^n}\sqrt{-1}}\right\} \cdot \left(2\sqrt{-1}\right)^{-1}\left\{\varepsilon^{\frac{x}{2^n}\sqrt{-1}} - \varepsilon^{-\frac{x}{2^n}\sqrt{-1}}\right)$.

Which is the analytical translation of $\sin. x = 2^{n} \cdot \cos.\frac{x}{2} \cdot \cos.\frac{x}{4}$ &c.

EULER, and after him other authors, have demonstrated these formulas by the aid of logarithms, and of theorems drawn from geometry.

X. EULER and LAGRANGE have treated certain differential equations, which are said to admit for their complete integration an algebraic form, although the integration of each particular term depends on the quadrature of the circle and hyperbola. I purpose to integrate these differential equations, by the method adopted in Articles V. VI.

Let fx, fy, denote functions of x and y.

Suppose the differential equation to be

$\frac{x'}{x} + \frac{y'}{y} = 0$; then $fx + fy = a$ when $x = \varepsilon^{fx}$, $y = \varepsilon^{fy}$. Hence, $xy = \varepsilon^{fx+fy} = \varepsilon^{a} = \mathrm{A}$, a constant quantity.

2dly. Let $\frac{x'}{\sqrt{1-x^2}} + \frac{y'}{\sqrt{1-y^2}} = 0$

$\therefore fx + fy = a$, x being $= \left\{2\sqrt{-1}\right\}^{-1} \cdot \left(\varepsilon^{fx\sqrt{-1}} - \varepsilon^{-fx\sqrt{-1}}\right)$,

and $y = 2\sqrt{-1})^{-1} \cdot (\varepsilon^{fy\sqrt{-1}} - \varepsilon^{-fy\sqrt{-1}})$; or $\sqrt{(1-x^2)} = 2^{-1} \cdot (\varepsilon^{fx\sqrt{-1}} + \varepsilon^{-fx\sqrt{-1}})$, and $\sqrt{1-y^2} = 2^{-1} \cdot (\varepsilon^{fy\sqrt{-1}} + \varepsilon^{-fy\sqrt{-1}})$.

Hence, $x \cdot \sqrt{(1-y^2)} + y\sqrt{(1-x^2)}$

$= (2\sqrt{-1})^{-1} \cdot \left\{\varepsilon^{(fx+fy)\sqrt{-1}} - \varepsilon^{-(fx+fy)\sqrt{-1}}\right\}$

$= (2\sqrt{-1})^{-1} \cdot \left\{\varepsilon^{a\sqrt{-1}} - \varepsilon^{-a\sqrt{-1}}\right\} = \mathrm{A}$, a constant quantity.

3dly. Let $\frac{x'}{\sqrt{a+bx+cx^2}}+\frac{y'}{\sqrt{a+by+cy^2}}=0$

$$\therefore \frac{x'}{\sqrt{c}\sqrt{x^2+\frac{bx}{c}+\frac{a}{c}}}+\frac{y'}{\sqrt{c}\sqrt{(y^2+\frac{by}{c}+\frac{a}{c})}}=0.$$

Let $\alpha+\frac{b}{2c}=v, y+\frac{b}{2c}=v'$ and $r^2=\frac{a}{c}-\frac{b^2}{4c^2}$

$$\therefore \frac{v'}{\sqrt{c}\sqrt{v^2+r^2}}+\frac{v'}{\sqrt{c}\sqrt{v'^2+r^2}}=0,$$

taking the integrals

$$c^{-\frac{1}{2}}\left\{V+V'\right\}=a, v=\frac{\varepsilon^V-r^2\varepsilon^{-V}}{2}, v'=\frac{\varepsilon^{V'}-r^2\varepsilon^{-V'}}{2}$$

$$\therefore v\sqrt{r^2+v^2}+v'\sqrt{(r^2+v^2}=\frac{\varepsilon^{V+V'}-r^2\varepsilon^{-(V+V')}}{2}=\frac{\varepsilon^{a\sqrt{c}}-r^2\varepsilon^{-a\sqrt{c}}}{2}$$

$=A$, and restoring the values of x and y,

$$\frac{2cx+b}{c}\sqrt{(a+by+cy^2)}+\frac{2cy+b}{c}\sqrt{(a+bx+cx^2)}=A'.$$

. By the above operation it appears, that certain algebraical expressions, as $x\sqrt{1-y^2}+y\sqrt{1-x^2}$, $\frac{2cx+b}{c}\sqrt{a+by+cy^2}$ &c. may be deduced, which answer the equations $\int\frac{x'}{\sqrt{1-x^2}}+\int\frac{y'}{\sqrt{1-y^2}}$ &c. But, strictly speaking, such algebraical expressions are not the integrals: they are rather expressions deduced from the true integral equations, from which other algebraical expressions, besides those put down, might be deduced.*

* For the integration of this sort of differential equations, see Mem. de Turin. Vol. IV. p. 98. "Sur l'Integration de quelques Equations differentielles, dont les indeterminées sont separées, mais dont chaque Membre en particulier n'est point integrable." In this Memoir are given three different methods of integrating $x^r(1-x^s)^{-\frac{t}{s}}=y^r(1-y^s)^{-\frac{t}{s}}$; by circular arcs and certain trigonometrical theorems, by impossible logarithms, and by partial integrations. Strictly speaking, all these methods are indirect; and the two first are only different but circuitous modes of expressing the method given in Art. X. See likewise Euler, Calc. integral. Vol. II. Novi Comm. Petrop. Tom. VI. p. 37. Tom. VII. p. 1. It is to be observed, that in the present state of analytic science, there is no certain and direct method of integrating differential equa-

XI. In the irreducible case of cubic equations, the root, it is said, may be exhibited by means of certain lines drawn in a circle. There is, however, independently of all geometrical considerations, a method of analytically expressing the root; and, from the analytical expression, although it is not the formula which from the time of CARDAN mathematicians have been seeking, the value of the root may in all cases be arithmetically computed; but, previously, it is necessary to shew what are the different symbols that may be substituted for z in the equations, $x = (2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}}\right\}$, and $\sqrt{(1-x^2)} = 2^{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}$. Let $x = 1$, and π be the value of z that answers the equations $1 = (2\sqrt{-1})^{-1}\left\{\varepsilon^{\pi\sqrt{-1}} - \varepsilon^{-\pi\sqrt{-1}}\right\}$ and $0 = 2^{-1}(\varepsilon^{\pi\sqrt{-1}} + \varepsilon^{-\pi\sqrt{-1}})$, which value of π may be numerically computed from the expression $\ldots \pi = z = x + \frac{x^3}{2.3} + \frac{3x^5}{5.8} + \frac{5x^7}{7.16}$ + &c. $(x = 1)$.

Hence, $\varepsilon^{\pi\sqrt{-1}} = -\varepsilon^{-\pi\sqrt{-1}} = \sqrt{-1}$ $\therefore \varepsilon^{2\pi\sqrt{-1}} = \varepsilon^{-2\pi\sqrt{-1}} = -1$ $\therefore \varepsilon^{4\pi\sqrt{-1}} = \varepsilon^{-4\pi\sqrt{-1}} = 1$ $\therefore \varepsilon^{8\pi\sqrt{-1}} = \varepsilon^{-8\pi\sqrt{-1}} = 1$ $\therefore$ $\varepsilon^{16\pi\sqrt{-1}} = \varepsilon^{-16\pi\sqrt{-1}} = 1$ &c. (for since $\varepsilon^{-m\pi\sqrt{-1}} = \frac{1}{\varepsilon^{m\pi\sqrt{-1}}}$, and $\varepsilon^{m\pi\sqrt{-1}} = \varepsilon^{-m\pi\sqrt{-1}} = \frac{1}{\varepsilon^{m\pi\sqrt{-1}}}$ $\therefore \varepsilon^{2m\pi\sqrt{-1}} = 1$).

Again, since $\varepsilon^{4\pi\sqrt{-1}} = 1$ and $\varepsilon^{8\pi\sqrt{-1}} = 1$, $\varepsilon^{12\pi\sqrt{-1}} = 1$; and

tions such as $x\left\{a+bx+cx^2+dx^3+ex^4\right\}^{-\frac{1}{2}} + y\left\{a+by+cy^2+dy^3+ey^4\right\}^{-\frac{1}{2}} = 0$, because no analytical expression or equation of a finite form has hitherto been invented, from which, according to the processes of the differential Calculus, such differential equations may be deduced. To find the algebraical expressions which answer to these equations, recourse must be had to what are properly to be depominated artifices. For such, see Mem. de Turin. Vol. IV. Comm. Petr. Tom. VI. VII. LAGRANGE, Fonct. Analyt. p. 80. LACROIX, Calc. diff. p. 427, &c.

generally $\varepsilon^{4n\pi\sqrt{-1}}=\varepsilon^{-4n\pi\sqrt{-1}}=1$, n any number of the progression 0, 1, 2, 3, 4, &c.

And, since $\varepsilon^{2\pi\sqrt{-1}}=\varepsilon^{-2\pi\sqrt{-1}}=-1 \therefore \varepsilon^{2\pi\sqrt{-1}}\times\varepsilon^{4n\pi\sqrt{-1}}=\varepsilon^{-2\pi\sqrt{-1}}\times\varepsilon^{-4n\pi\sqrt{-1}}=-1$, or $\varepsilon^{(2n+1)\,2\pi\sqrt{-1}}=\varepsilon^{-(2n+1)2\pi\sqrt{-1}}=-1$, n any number of the progression 0, 1, 2, 3, 4, 5, &c.

Hence it appears, that if $x=(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}$, $x\times 1=\left\{2\sqrt{-1}\right\}^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}\times\varepsilon^{4n\pi\sqrt{-1}}=$ (since $\varepsilon^{4n\pi\sqrt{-1}}=\varepsilon^{-4n\pi\sqrt{-1}}$) $(2\sqrt{-1})^{-1}\left\{\varepsilon^{(4n\pi+z)\sqrt{-1}}-\varepsilon^{-(4n\pi+z)\sqrt{-1}}\right\}$.

Again, since $\varepsilon^{(2n+1)2\pi\sqrt{-1}}=\varepsilon^{-(2n+1)\,2\pi\sqrt{-1}}=-1$

$x\times-1=(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}\varepsilon^{(2n+1)\pi\sqrt{-1}}$

$=(2\sqrt{-1})^{-1}\left\{-\varepsilon^{((2n+1)2\pi-z)\sqrt{-1}}-\varepsilon^{-((2n+1)2\pi-z)\sqrt{-1}}\right.$,

consequently,

$x=(2\sqrt{-1})^{-1}\left\{\varepsilon^{((2n+1)2\pi-z)\sqrt{-1}}-\varepsilon^{-((2n+1)2\pi-z)\sqrt{-1}}\right\}$,

or the equation $x=(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}$ is true, when instead of z is put $(4\pi+z)$ or $(8\pi+z)$, or generally $(4n\pi+z)$; and is moreover true, when instead of z is put

$(2\pi-z)$, $(6\pi-z)$, or generally $(2n+1)\,2\pi-z$.

In like manner, the equation $\sqrt{1-x^2}=2^{-1}\left\{\varepsilon^{z\sqrt{-1}}+\varepsilon^{-z\sqrt{-1}}\right\}$ is true, when instead of z is put

$4\pi+z$, $8\pi+z$, or $12\pi+z$, or generally $4n\pi+z$;

and is moreover true, when instead of z is put

$4\pi-z$, $8\pi-z$, or $12\pi-z$, or generally $4n\pi-z$.

Let now $x^3-qx=r$, then, by CARDAN's solution,

$$x=\sqrt[3]{\left(\frac{r}{2}+\sqrt{\left(\frac{r^2}{4}-\frac{q^3}{27}\right)}\right)}+\sqrt[3]{\left(\frac{r}{2}-\sqrt{\left(\frac{r^2}{4}-\frac{q^3}{27}\right)}\right)};$$

put $\frac{r}{2}=a$, $\frac{r^2}{4}-\frac{q^3}{27}=-b^2$, then $x=\sqrt[3]{(a+b\sqrt{-1})}+\sqrt[3]{a(-b\sqrt{-1})}$.

Let $a+b\sqrt{-1}=m\varepsilon^{z\sqrt{-1}} \therefore a-b\sqrt{-1}=m\varepsilon^{-z\sqrt{-1}}$

$a^2+b^2=m^2$, $a=m\left\{\frac{\varepsilon^{z\sqrt{-1}}+\varepsilon^{-z\sqrt{-1}}}{2}\right\}$, $b=m\left\{\frac{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}}{2\sqrt{-1}}\right\}$, or $2^{-1}.\left\{\varepsilon^{z\sqrt{-1}}+\varepsilon^{-z\sqrt{-1}}\right\}=\frac{a}{\sqrt{a^2+b^2}}$, and $(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}=\frac{b}{\sqrt{a^2+b^2}}$; but, from what has been premised, these equations are true, when instead of z is put $\theta+z$, or $2\theta+z$, or $4\theta+z$, or generally $n\theta+z$, $(4\pi=\theta)$.

Hence, $\sqrt[3]{(a+b\sqrt{-1})}+\sqrt[3]{(a-b\sqrt{-1})}$ is $m^{\frac{1}{3}}\left\{\varepsilon^{\frac{z}{3}\sqrt{-1}}+\varepsilon^{-\frac{z}{3}\sqrt{-1}}\right\}$, or $m^{\frac{1}{3}}\left\{\varepsilon^{\frac{\theta+z\sqrt{-1}}{3}}+\varepsilon^{\frac{-\theta+z\sqrt{-1}}{3}}\right\}$, or generally $m^{\frac{1}{3}}\left\{\varepsilon^{\frac{n\theta+z\sqrt{-1}}{3}}+\varepsilon^{\frac{-(n\theta+z)\sqrt{-1}}{3}}\right\}$: there are, however, only 3 different values of x, for the index of ε in the fourth value is $\frac{3\theta+z\sqrt{-1}}{3}$, and $\varepsilon^{\frac{3\theta+z\sqrt{-1}}{3}}=\varepsilon^{\theta\sqrt{-1}}\times\varepsilon^{\frac{z}{3}\sqrt{-1}}=1\times\varepsilon^{\frac{z}{3}\sqrt{-1}}$. ∴ the fourth value is the same as the first. Again, the index of ε in the fifth value is $\frac{4\theta+z}{3}\sqrt{-1}$; but $\varepsilon^{\frac{(4\theta+z)}{3}\sqrt{-1}}=\varepsilon^{\theta\sqrt{-1}}\times\varepsilon^{\frac{\theta+z}{3}\sqrt{-1}}=1\times\varepsilon^{\frac{\theta+z}{3}\sqrt{-1}}$. The 5th value is the same as 2d, and so on; and, consequently, the indices of e in the 3 different values of x are $\pm\frac{z}{3}\sqrt{-1}$, $\pm\frac{\theta+z}{3}\sqrt{-1}$, $\pm\frac{2\theta+z}{3}\sqrt{-1}$.

If, instead of the index of ε in the 3d value, $\pm\frac{\theta-z}{3}\sqrt{-1}$ be put, the value of the root remains the same; for, since $\varepsilon^{\theta\sqrt{-1}}=\varepsilon^{-\theta\sqrt{-1}}=1$ ∴ $x\times1=m^{\frac{1}{3}}\times\left\{\varepsilon^{\frac{2\theta+z}{3}\sqrt{-1}}\times\varepsilon^{-\theta\sqrt{-1}}+\varepsilon^{\frac{-2\theta+z}{3}\sqrt{-1}}\times\varepsilon^{\theta\sqrt{-1}}\right\}=m^{\frac{1}{3}}\left\{\varepsilon^{\frac{-\theta-z}{3}\sqrt{-1}}+\varepsilon^{\frac{\theta-z}{3}\sqrt{-1}}\right\}$.

This mode of representing the roots is not, as has been

already stated, according to the conditions* of the formula demanded by mathematicians. It enables us, however, immediately to ascertain that the roots are possible, and to calculate their approximate value; for, when $\sqrt{1-x^2}$ or $y = 2^{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}$, $z = \int -\frac{y}{\sqrt{1-y^2}}$

$= \alpha - \left\{ y + \frac{y^3}{3.2} + \frac{3y^5}{5.8} + \frac{5y^7}{7.16} + \text{&c.} \right\}$,

when $z = o$ $y = 2^{-1}\left\{\varepsilon^{o} + \varepsilon^{-o}\right\} = 1$

$\therefore \alpha = 1 + \frac{1}{3.2} + \frac{3}{5.8} + \frac{5}{7.16} + \text{&c.}\} = \pi.$

Hence, we may numerically approximate to the value of z from the expression $z = \pi - \left\{ y + \frac{y^3}{3.2} + \frac{3y^5}{5.8} + \text{&c.}\right\}$ when y is given, and < 1. Now, in the case of the cubic equation, $y = \frac{a}{\sqrt{a^2+b^2}} = \frac{3r\sqrt{3}}{2q\sqrt{q}}$; and, since $\frac{r^2}{4} < \frac{q^3}{27}$ $\therefore \frac{3r\sqrt{3}}{2q\sqrt{q}}$ is < 1, consequently the value of z may be obtained; suppose it t, then the roots are to be approximated to, by means of the series that result from the developements of the forms by which they are represented; to wit,

$$2\sqrt{\frac{q}{3}}\left\{1 - \frac{t^2}{1.2.3^2} + \frac{1}{1.2.3.4}\cdot\frac{t^4}{3^4} - \text{&c.}\right\}$$

$$2\sqrt{\frac{q}{3}}\left\{1 - \frac{(\theta+t)^2}{1.2.3^2} + \frac{1}{1.2.3.4}\cdot\frac{(\theta+t)^4}{3^4} - \text{&c.}\right\}$$

$$2\sqrt{\frac{q}{3}}\left\{1 - \frac{(2\theta+t)^2}{1.2.3^2} + \frac{1}{1.2.3.4}\cdot\frac{(2\theta+t)^4}{3^4} - \text{&c.}\right\}$$

Now these series converge; for, since t is finite, we must at length arrive at a term $\frac{A}{n}$, in which $(n-1)\,n$ is $> \left(\frac{t}{3}\right)^2$; and, since $(n+1)$th term $\left(\frac{A}{n+1}\right)$ is $= \frac{A}{n} \times \left(\frac{t}{3}\right)^2 \times \frac{1}{n+1\,(n+2)}$ $\therefore \frac{A}{n+1}$ is

* The conditions of the formula are, that it should be finite in regard to the number of terms, free from imaginary quantities, and containing only the coefficients q and r. See Mem. de l'Acad. 1738.

$< \frac{A}{n}$, a fortiori, $\frac{A}{n+1}$ is $< \frac{A}{n+1}$, and so on; the terms after the $n-1$th term constantly diminishing.*

The above method is purely analytical: it has no tacit reference to other methods; it does not virtually suppose the existence either of an hyperbola or circle. If practical commodiousness, however, be aimed at, it is *convenient* to give a different expression to the values of the roots, or to translate them into geometrical language: and this, because tables have been calculated, exhibiting the numerical values of the cosines, &c. of circular arcs. Now, since it has already appeared that the cosine of an arc $z = 2^{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\}$, the 3 roots of the equation $x^3 - qx = r$ may be said to equal

$$2\sqrt{\frac{q}{3}} \cdot \cos.\ \frac{t}{3},\ 2\sqrt{\frac{q}{3}} \cos.\ \frac{\theta+t}{3},\ 2\sqrt{\frac{q}{3}},\ \cos.\ \frac{\theta-t}{3}.$$

XII. In the fifth volume of his *Opuscules*,† D'ALEMBERT

* In the Phil. Trans. for 1801. p. 116, I mentioned M. NICOLE as the first mathematician who shewed the expression of the root in the irreducible case, when expanded, to be real. But the subjoined passage, in LEIBNITZ's Letter to WALLIS, causes me to retract my assertion. "Diu est quod ipse quoque judicavi $\sqrt[3]{a+b\sqrt{-1}}$ "$+\sqrt[3]{a+b\sqrt{1}} = z$ esse quantitatem realem, etsi speciem habeat imaginariæ; "ob virtualem nimirum imaginariæ destructionem, perinde ac in destructione actuali "$a+b\sqrt{-1} + a-b\sqrt{-1} = 2a$. Hinc, si ex $\sqrt[3]{a\pm b\sqrt{-1}}$ extrahamus radicem "ope seriei infinitæ, ad inveniendum valorem ipsius z serie tali expressum, efficere "possumus, ut reapse evanescat imaginaria quantitas. Atque ita etiam, in casu ima-"ginario, regulis Cardanicis cum fructu utimur," &c. Vol. III. p. 126. See also p. 54.

† "Elle etoit neanmoins d'autant plus essentielle, que l'expression de l'arc par le "sinus, fondée sur la serie connue, qui est l'intégrale de $\frac{dx}{\sqrt{1-x^2}}$, ne peut etre regardée "comme exacte, c'est a dire, comme representant à la fois tous les arcs qui ont le "même sinus; puisque cette serie ne represente evidemment qu'un seul des arcs qui "repondent au sinus dont il s'agit, savoir, le plus petit de ces arcs, celui qui est infé-"rieur, ou tout au plus egal, a 90 degrés. Cependant, c'est d'un autre côté une sorte "de paradoxe remarquable, que l'expression de l'arc par le sinus ne representant qu'un

mentions it as a remarkable paradox, that the series for the arc in terms of the sine represents only one arc, viz. the arc less than 90 degrees; whereas the series for the sine, produced by reversion from the former series, exhibits all possible arcs that have the same sine. I shall endeavour to solve this paradox, which, I think, originated partly from the introduction of geometrical considerations into an analytical investigation, by which the true derivation of certain expressions was concealed.

It has appeared that the equation $x=(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}$ is true, when instead of z is put, $\theta+z$, or $2\theta+z$, ... or $n\theta+z$,

or $\frac{\theta}{2}-z$, or $\frac{3\theta}{2}-z$ .... or $\frac{2n+1}{2}\theta-z$.

Now, if the fluxions of these equations are taken, and the equations cleared of exponential quantities, there results from each the same equation, to wit, $z\cdot=\frac{x\cdot}{\sqrt{1-x^2}}$. Hence, if the symbol $\int$ denotes the operation by which we are to ascend to the original equations from which $z\cdot=\frac{x\cdot}{\sqrt{1-x^2}}$ is derived, the only strict consequence from $\int z\cdot=\int\frac{x\cdot}{\sqrt{1-x^2}}$

is that $x=(\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\}$, or $=(2\sqrt{-1})^{-1}$

$\left\{\varepsilon^{(\theta+z)\sqrt{-1}}-\varepsilon^{-(\theta+z)\sqrt{-1}}\right\}$,

or generally $=(2\sqrt{-1})^{-1}\left\{\varepsilon^{(n\theta+z)\sqrt{-1}}-\varepsilon^{-(n\theta+z)\sqrt{-1}}\right\}$, or

$=(2\sqrt{-1})^{-1}\left\{\varepsilon^{\frac{2n+1}{2}\theta\sqrt{-1}}-\varepsilon^{\frac{-(2n+1)}{2}\theta\sqrt{-1}}\right\}$.

" seul arc de 90 degrés au plus, l'expression du sinus par l'arc, qu'on peut deduire (par " la méthode du retour de suites) de l'expression de l'arc par le sinus, represente " exactement, etant poussée a l'infini, le sinus de tous les arcs possibles, plus petits " ou plus grands que 90°, et même que la circonference ou demi circonference, prise " tant de fois qu'on voudra. Je laisse à d'autres geometres, le soin d'éclaircir ce " mystère, ainsi que plusieurs autres," &c. p. 183.

Hence, to answer the equation $z = \frac{x^n}{\sqrt{1-x^2}}$,

x may $= z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} - \&c.$

or $z' - \frac{z'^3}{1.2.3} + \frac{z'^5}{1.2.3.4.5} - \&c.$

or $z'' - \frac{z''^3}{1.2.3} + \frac{z''^5}{1.2.3.4.5} - \&c$

$\{z', z'', z''', \&c.$ representing $\theta+z$, $2\theta+z$, $3\theta+z$, $\&c.\}$.

Suppose now it is necessary to deduce z, z', z'', &c. in terms of x and its powers, by reversion of series. What does the reversion of series mean? Merely this; a certain method or operation, according to which, one quantity being expressed in terms of another, the second may be expressed in terms of the first. Hence, in all similar series, the operation must be the same; consequently, the result, which is merely the exhibition of a formula, must be the same; so that, whatever is the series in terms of x, produced by *reversion* in

$x = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} - \&c.$ the same must be produced by reversion in $x = z' - \frac{z'^3}{1.2.3} + \frac{z'^5}{1.2.3.4.5} - \&c.$

in $x = z'' - \frac{z''^3}{1.2.3} + \&c.$

&c.

The series produced by reversion in these cases is, $x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} + \&c.$ Hence it appears, that we know, a priori, that must happen which D'ALEMBERT considers as a paradox to have happened. Why this paradox found reception in the mind of this acute mathematician, I have stated, as my opinion, one cause to have been, an inattention, from geometrical considerations, to the real origin and derivation of certain expressions that appeared in the course of the calculation. Another cause I apprehend was, the want of precise notions on the force and

signification of the symbol $=$. It is true that its signification entirely depends on definition; but, if the definition given of it in elementary treatises be adhered to, I believe it will be impossible to shew the justness and legitimacy of most mathematical processes. It scarcely ever denotes numerical equality. In its general and extended meaning,* it denotes the result of certain operations. Thus, when from

$x = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5}$, $x = z' - \frac{z'^3}{1.2.3}$ &c.

z or z' is inferred $= x + \frac{x^3}{3.2} + \frac{3x^5}{5.8}$ &c. nothing is affirmed concerning a numerical equality; and all that is to be understood is, that $x + \frac{x^3}{3.2} + \frac{3x^5}{5.8}$ + &c. is the result of a certain operation performed on $x = z - \frac{z^3}{1.2.3} + \frac{z^5}{1.2.3.4.5} -$ &c.

XIII. It appears then, that according to the reversion of series, z, z', z'', &c. must all be represented by the same series, proceeding according to the powers of x; but, if a form for z be required, which shall in all cases afford us a means of numerically computing its value, such a form must involve certain arbitrary quantities. These arbitrary quantities are to be determined by conditions which depend either on the original form of the equation between x and z, or on the nature of the object to which the calculus is applied.

Let now $\int \frac{x^r}{\sqrt{1-x^2}}$ mean† $x + \frac{x^3}{3.2} + \frac{3x^5}{5.8}$ + &c.

* This is consistent with what I advanced in the Phil. Trans. for 1801. p. 99, concerning the meaning of the symbols $\times$ $+$, &c. It is beside my present purpose, to insist farther on the necessity of attaching precise notions to the symbols employed in calculation; and the subject deserves a separate and ample discussion.

† It is not so easy to prove as it may be imagined, that $\int \frac{x^r}{\sqrt{1-x^2}} \cdot = x + \frac{x^3}{3.2} + \frac{3x^5}{5.8}$ + &c.

then, if z represent the arc of a circle, and x the sine, this equality* $z = x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} +$ &c. is subject to restrictions, for x cannot exceed 1; consequently, the greatest value of z that can be determined from the equation, must be so determined by putting $x = 1$. Let $\pi = 1 + \frac{1}{3.2} + \frac{3}{5.8}$ &c.

Now, from the definition of sine and the nature of the circle, the arcs $2\pi - z$, $6\pi - z$ .... $(2n+1)\,2\pi - z$ .... $4\pi + z$ .... $8\pi + z$ .... $4n\pi + z$, have the same sine; let these arcs be z, z', z'', z''', &c. and let $x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} +$ &c. $= X$,

then $z' = 2\pi - X$, $z'' = 6\pi - X$, &c. or generally

$z^{n \cdots n} = \left\{2n+1\right\} 2\pi - X$, or $= 4n\pi + X$,

n any number of the progression 0, 1, 2, 3, 4, &c.

Or thus, from the conditions contained in the form of the equation between z and x,

since $\sqrt{1-x^2} = 2^{-1} \cdot \left\{ \varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}} \right\} = 1 - \frac{z^2}{1.2} +$ &c.

there is no possible value of z that answers the equation when x is > 1,

Let $\int \frac{x}{\sqrt{1-x^2}} = X + a$ $\therefore$ when z and x begin together,

$a = 0$ and $z = X$.

But the equation $\frac{x}{\sqrt{1-x^2}} = z$ may be derived from $x = (2\sqrt{-1})^{-1} \left\{ \varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}} \right\}$,

when instead of z is put $2\pi - z$, $6\pi - z$ .... $(2n+1)\,2\pi - z$,

* In the expression $z = x + \frac{x^3}{3.2} + \frac{3x^5}{5.8} +$ &c. considered abstractedly from its origin and application, there is nothing that limits the value of x. Again, by applying the operation of reversion, x is represented by this form, $x - \frac{x^3}{1.2.3} + \frac{x^5}{1.2.3.4.5}$ &c. But there is no method, I believe, of proving (I purposely exclude that unproved proposition that every equation has as many roots as dimensions) that instead of z in $x - z + \frac{z^3}{1.2.3} -$ &c. $= 0$, other quantities, as z', z'', &c. may be substituted.

or $4\pi+z$ .... $4n\pi+z$.

Hence, z' or $2\pi-z=-X+\alpha$. Let $z=0$ $\therefore$ $X=0$ $\therefore$ $2\pi=\alpha$.
Again, z'' or $6\pi-z=-X+\alpha$. Let $z=0$ $\therefore$ $X=0$ $\therefore$ $6\pi=\alpha$.
Hence, the arbitrary quantity α may generally be represented by $(2n+1)2\pi$, or by $4n\pi$ $\therefore$ $z''\cdots''=(2n+1)2\pi-X$,

or $=4n\pi+X$.

XIV. I shall now shew, by a purely analytical process, what are the divisors of $x^n\mp a^n$. It seems a very strange and absurd method, to refer to the properties of geometrical figures, for the knowledge of the composition of analytical expressions.

Let $x=m^{\frac{1}{n}}\varepsilon^{\frac{z}{n}\sqrt{-1}}$ $\therefore$ $a^n=m\varepsilon^{z\sqrt{-1}}$ $\therefore$ $m=\frac{a^n}{\varepsilon^{z\sqrt{-1}}}$, and m will be always positive, if $\varepsilon^{z\sqrt{-1}}=1$. But (Art. XI.) the values of z that answer the equation $\varepsilon^{z\sqrt{-1}}=1$, are $o\theta,\pm\theta,\pm 2\theta$, $\pm 3\theta$, or generally $\pm s\theta$, s, any number of the progression 0, 1, 2, 3, &c.

Hence, $x=a\varepsilon^{\frac{\pm s\theta}{n}\sqrt{-1}}$ generally,

or values of x are a, $a\varepsilon^{\frac{\theta}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{-\theta}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{2\theta}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{-2\theta}{n}\sqrt{-1}}$, &c.

$\therefore x^n-a^n=(x-a)\left(x^2-a\left\{\varepsilon^{\frac{\theta}{n}\sqrt{-1}}+\varepsilon^{\frac{-\theta}{n}\sqrt{-1}}\right\}+a^2\right)\left(x^2-a\left\{\varepsilon^{\frac{2\theta}{n}\sqrt{-1}}+\varepsilon^{\frac{-2\theta}{n}\sqrt{-1}}\right\}+a^2\right)$, &c. n being odd;

when n is even, (and of the form $2p$, p odd,) there must be a number (s) in the progression (0, 1, 2, 3, &c.) that $=\frac{n}{2}$;

consequently, there must be a value of x, $a\varepsilon^{\frac{s\theta}{n}\sqrt{-1}}=a\varepsilon^{\frac{\theta}{2}\sqrt{-1}}$
$=-a$, since (Art. XI.) $\varepsilon^{2\pi\sqrt{-1}}$, or $\varepsilon^{\frac{\theta}{2}\sqrt{-1}}=-1$.

Hence, a quadratic divisor of x^n-a^n will be $(x-a).(x+a)$, or x^2-a^2; when n is even, and of the form $4p$, p even or odd,

there must be a number (s) in the progression ($0, 1, 2, 3 \ldots.$) $= \frac{n}{4}$; consequently, there must be a value of x, $a\varepsilon^{\frac{\pm s\theta}{n}\sqrt{-1}} = a\varepsilon^{\frac{\pm\theta}{4}\sqrt{-1}} = a \times \pm\sqrt{-1}$, since (Art. XI.) $\varepsilon^{\pm\pi\sqrt{-1}}$, or $\varepsilon^{\frac{\pm\theta}{4}\sqrt{-1}} = \pm\sqrt{-1}$.

Hence, one quadratic divisor of $x^n - a^n$ will be of the form $x^2 + a^2 = (x + a\sqrt{-1}).\ (x - a\sqrt{-1})$; another, as it has been already shewn, will be of the form $x^2 - a^2$.

There are only n different divisors, for (n odd) the $(n-1)$th and nth divisors are comprised under the form $x = a\varepsilon^{\frac{\pm n-1}{2n}\theta\sqrt{-1}}$; the succeeding divisors would be comprised under the form

$$x = a\varepsilon^{\frac{\pm n+1}{2n}\theta\sqrt{-1}} = a\varepsilon^{\pm\theta\sqrt{-1}} \times \varepsilon^{\frac{\mp n-1}{2n}\theta\sqrt{-1}}$$

$= a\varepsilon^{\frac{\mp n-1}{2n}\theta\sqrt{-1}}$, (since $\varepsilon^{\pm\theta\sqrt{-1}} = 1$) the same as preceding form.

If $x^n + a^n = o$, then $m = \frac{-a^n}{\varepsilon^{n\sqrt{-1}}}$, to have m always positive.

Let $\varepsilon^{z\sqrt{-1}} = -1$, then (Art. XI.) the values of z are $\pm 2\pi, \pm 6\pi \ldots$ &c.

Let $2\pi = \rho$, then generally $\varepsilon^{(2s+1)\rho\sqrt{-1}} = -1$; consequently, $x = a\varepsilon^{\frac{\pm(2s+1)}{n}\rho\sqrt{-1}}$, s any number of the progression 0, 1, 2, &c.

or the values of x are $a\varepsilon^{\frac{\pm\rho}{n}\sqrt{-1}}, a\varepsilon^{\frac{\pm 3\rho}{n}\sqrt{-1}}$ &c.

or $x^n + a^n = (x^2 - a\left\{\varepsilon^{\frac{\rho}{n}\sqrt{-1}} + \varepsilon^{\frac{-\rho}{n}\sqrt{-1}}\right\} + a^2) \cdot (x^2 - a\left\{\varepsilon^{\frac{3\rho}{n}\sqrt{-1}} + \varepsilon^{\frac{-3\rho}{n}\sqrt{-1}}\right\} + a^2)$ &c.

When n is odd, there must be a number ($2s+1$) in the progression ($1, 3, 5, 7,$ &c.) $= n$; consequently, one value of x must

$= a\varepsilon^{\pi\sqrt{-1}} = -a$, or $x+a$ must be a divisor of x^n+a^n.

XV. Resolution of $x^{2n}-2lx^n a^n+a^{2n}$ into its quadratic factors $l \angle 1$.

Now, from the equation $x^n = a^n\left\{l \pm \sqrt{l^2-1}\right\} = A \pm B\sqrt{-1}$.

Let $x = m^{\frac{1}{n}} \varepsilon^{\frac{\pm z}{n}\sqrt{-1}}$. $\therefore m\varepsilon^{z\sqrt{-1}} = A+B\sqrt{-1}$, $m\varepsilon^{-z\sqrt{-1}} = A-B\sqrt{-1}$, $m = \sqrt{(A^2+B^2)}$, $2^{-1}\left\{\varepsilon^{z\sqrt{-1}}+\varepsilon^{-z\sqrt{-1}}\right\} = \frac{A}{\sqrt{A^2+B^2}} = l$,

$(2\sqrt{-1})^{-1}\left\{\varepsilon^{z\sqrt{-1}}-\varepsilon^{-z\sqrt{-1}}\right\} = \frac{B}{\sqrt{A^2+B^2}} = \sqrt{1-l^2}$;

but (Art. XI.) these equations are true, when instead of z are put $\theta+z$, $2\theta+z$, $3\theta+z$ .... generally $s\theta+z$.

Hence, the general value of x is $a\varepsilon^{\frac{\pm s\theta+z}{n}\sqrt{-1}}$, and the values of x are $a\varepsilon^{\frac{\pm z}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{\pm\theta+z}{n}\sqrt{-1}}$, $a\varepsilon^{\frac{\pm 2\theta+z}{n}\sqrt{-1}}$,

or $x^{2n}-2la^n x^n+a^{2n} = \left(x^2-a\left\{\varepsilon^{\frac{z}{n}\sqrt{-1}}+\varepsilon^{\frac{-z}{n}\sqrt{-1}}\right\}+a^2\right)\times\left(x^2-a\left\{\varepsilon^{\frac{\theta+z}{n}\sqrt{-1}}+\varepsilon^{\frac{-\theta+z}{n}\sqrt{-1}}\right\}+a^2\right)\times$ &c.

XVI. Such are the analytical processes according to which the resolutions of $x^n \mp a^n$, $x^{2n} \mp 2la^n x^n + a^{2n}$ are effected; and thence the fluents of $\frac{x^r}{x^n \mp a^n}$, $\frac{x^r}{x^{2n}-2la^n x^n+a^{2n}}$, &c. &c. may be obtained, by resolving the fractions $\frac{1}{x^n-a^n}$ &c. into a series of partial fractions, of the form $\frac{Ax+B}{x^2+2\alpha x+\alpha^2+\beta^2}$.

Since the above resolution of $x^n \mp a^n$ into its quadratic factors would, it appears to me, be strictly true, if such a curve as the circle had never been invented, nor its properties investigated, it is erroneous to suppose that the theorem of Cotes is essentially necessary for the integration of certain differential forms.

That analytical science was advanced by the discovery of this theorem, is indeed true; but the circle and its lines were no farther useful or necessary, than as they afforded a mode of expressing, in geometrical language, an analytical truth. What is analytically expressed, may be analytically combined and resolved; and, if COTES, by the properties of figures, has expressed his discovery, it is because the mathematicians of the time in which he lived, were more skilful and dexterous with the geometrical method than with the analytical.

In order to demonstrate COTES's property of the circle, considered as such, one of two different methods must be pursued. Either let the demonstration be strictly geometrical, according to the method of the ancients, or as completely analytical as possible; that is, let the demonstration be effected by the analytical method, from as few fundamental principles as possible. I know not on what grounds of perspicuity and rigour, the propriety of a demonstration half geometrical, half algebraical, can be established; for, besides the want of symmetry in such a demonstration, in strictness of reasoning, a separate discussion is necessary, to shew the propriety and justness of the application of analysis to certain properties of extension demonstrated geometrically.

It is beside my present purpose, to inquire whether COTES's theorem can be demonstrated strictly after the method of the ancients: hitherto it has not been so demonstrated. To demonstrate it analytically, in the most simple and direct manner, we must proceed from as few fundamental principles as possible;* and give to the quantities concerned, their true and natural

* For the analytical demonstration, all that is necessary to be known, is what is proved in the 47th of the Elements.

representation. I think, therefore, the analytical demonstration in which the symbol $\sqrt{-1}$ is introduced, (for the cosine of an arc cannot be adequately and abridgedly represented in terms of the arc, except by means of the symbol $\sqrt{-1}$,) to be the most simple and direct that can be exhibited. I have endeavoured, in a former paper, to shew that demonstration with such symbols as $\sqrt{-1}$ may be strict and rigorous.

XVII. One or two more instances of the advantage accruing to calculation, from giving to quantities in analytical investigation their true analytical representation, I now offer, in the demonstrations of the series for the chord of the supplement of a multiple arc, in terms of the chord of the supplement of the simple arc, for the sine of the multiple arc, &c.

Chord $2\pi-z = (\sqrt{-1})^{-1}\left\{\varepsilon^{\frac{(2\pi-z)}{2}\sqrt{-1}} - \varepsilon^{\frac{-(2\pi-z)}{2}\sqrt{-1}}\right\}$,

$= \varepsilon^{\frac{z}{2}\sqrt{-1}} + \varepsilon^{\frac{-z}{2}\sqrt{-1}}$, since $\varepsilon^{\pi\sqrt{-1}} = \sqrt{-1}$, and $\varepsilon^{-\pi\sqrt{-1}} = -\sqrt{-1}$.

Again, chord $(2\pi-nz) = \varepsilon^{\frac{nz}{2}\sqrt{-1}} + \varepsilon^{\frac{-nz}{2}\sqrt{-1}}$. Let $\varepsilon^{\frac{z}{2}\sqrt{-1}} = \alpha$, $\varepsilon^{\frac{-z}{2}\sqrt{-1}} = \beta$ $\therefore \alpha\beta = 1$; what we have to do then, is to find $\alpha^n+\beta^n$ in terms of $\alpha+\beta$; and, for facility of computation, a new mode of notation may be advantageously introduced, which requires a brief explanation only.*

* I had obtained the forms for chords nz, &c. given in the following pages, by actually expressing in terms of n and b, the coefficient of x^n, in the developement of the trinomial $\left\{1+bx+x^2\right\}^m$, when the very admirable work of Arbogast, Du Calcul des Derivations, came to my hands. The great simplicity and convenience of his notation have caused me to adopt it, although it does not harmonize well with the fluxionary notation which I have employed in the present Paper.

By Art. V. $\varphi(x+o) = \varphi x + Po + \frac{Qo^2}{1.2} + \frac{Ro^3}{1.2.3}$ &c.

Let D be the note of the operation to be performed on φx, in order to deduce P, then $P = D\varphi x$, $Q = DP = DD\varphi x = D^2\varphi x$ &c.

Hence, $\varphi(x+o) = \varphi x + D\varphi x\, o + \frac{D^2\varphi x \cdot o^2}{1.2} +$ &c.

or, representing $\frac{D^n\varphi x}{1.2.3\ldots n}$ by $D_c^n\varphi x$,

$\varphi(x+o) = \varphi x + D\varphi x \cdot o + D_c^2\varphi x \cdot o^2 + D_c^3\varphi x \cdot o^3 +$ &c.

To resume the demonstration:

$\frac{1}{1+\alpha x} + \frac{1}{1+\beta x} = \frac{2+(\alpha+\beta)x}{1+(\alpha+\beta)x+\alpha\beta x^2} = \frac{2+bx}{1+bx+cx^2}$,

now $\frac{1}{1+\alpha x} = 1 - \alpha x + \alpha^2 x^2 \ldots\ldots \pm \alpha^n x^n \ldots$

and $\frac{1}{1+\beta x} = 1 - \beta x + \beta^2 x^2 \ldots\ldots \pm \beta^n x^n$

$\therefore$ term affected with x^n in developement of $\frac{1}{1+\alpha x} + \frac{1}{1+\beta x}$ is $\pm\{\alpha^n + \beta^n\}$.

Again, $\frac{2+bx}{1+bx+cx^2} = (2+bx)(1+bx+cx^2)^{-1}$

now $(1+bx)^{-1} = 1^{-1} + D1^{-1}.bx + D_c^2 1^{-1}.b^2x^2 + D_c^3 1^{-1}.b^3x^3 +$ &c.

for b put $b+cx$, and for b^m, $\{b+cx\}^m$, or $b^m + Db^m.cx + D_c^2 b^m.c^2x^2$ &c.

then

$$(1+bx+cx^2)^{-1} = 1^{-1} + D1^{-1}.bx + D1^{-1}.bx.cx + D_c^2 1^{-1}.b^2x^2 + D_c^2 1^{-1}.Db^2.cx^2 + D_c^2 1^{-1}c^2x^4 \text{ &c.} + D_c^3 1^{-1}.b^3x^3 + D_c^3 1^{-1}.Db^3.cx^4 \text{ &c.} + D_c^4 1^{-1}.b^4x^4 \text{ &c.}$$

Hence, terms affected with x^n and x^{n-1} are

$$D_c^n 1^{-1}.b^n + D_c^{n-1} 1^{-1}.Db^{n-1}c + D_c^{n-2} 1^{-1}.D_c^2 b^{n-2}c^2 + D_c^{n-3} 1^{-1}.D_c^3 b^{n-3}c^3 + \text{ &c.}$$

and $D_c^{n-1} 1^{-1}.b^{n-1} + D_c^{n-2} 1^{-1}.Db^{n-2}c +$ &c. Now, the mth term

from the beginning in first series, is $\mathrm{D}_c^{n-m} 1^{-1} . \mathrm{D}_c^m b^{n-m} c^m$;

which, n even and $m = \frac{n}{2} = \mathrm{D}_c^m 1^{-1} . c^m$

n odd and $m = \frac{n-1}{2} = \mathrm{D}_c^{m+1} 1^{-1} . \overline{m+1}\, b$.

At these terms the series terminates; all the succeeding terms being equal 0, since $\mathrm{D}^{m+1} b^m$, $\mathrm{D}^{m+2} b^{m-1}$, are respectively $= m . \overline{m-1}\, \overline{m-2} \ldots 3.2.1.0 = \overline{m-1}\, \overline{m-2} \ldots 3.2.1.0$ and $= 0$.

Hence, the series written in a reverse order is (n even)

$$\mathrm{D}_c^n 1^{-1} . c^n + \mathrm{D}_c^{n+1} 1^{-1} . \mathrm{D}_c^{n-1} . b^{n+1} . c^{n-1} + \mathrm{D}_c^{n+2} 1^{-1} . \mathrm{D}_c^{n-2} b^{n+2} . c^{n-2} \ldots \mathrm{D}_c^n 1^{-1} . b^n$$

(n odd)

$$\text{or } \mathrm{D}_c^{n+1} 1^{-1} . \mathrm{D}_c^n b^{n+1} . c^n + \mathrm{D}_c^{n+2} 1^{-1} . \mathrm{D}^{n-1} b^{n+2} . c^{n-1} + \&c. \ldots\ldots \mathrm{D}_c^n 1^{-1} . b^n$$

Now, $\mathrm{D}_c^n 1^{-1} = \frac{-1 . -2 . -3 \ldots -n}{1 . 2 . 3 \ldots n} = 1$ (n even) or $= -1$ (n odd)

and $\therefore$ the former series becomes

$\pm b_n \pm \mathrm{D} b^{n-1} . c \mp \mathrm{D}_c^2 b^{n-2} . c^2 \pm \&c.$ and consequently, the term affected with x^n in $(2 + bx)(1 + bx + cx^2)^{-1}$

is $\begin{cases} \pm 2 b^n \mp 2 \mathrm{D} b^{n-1} . c \pm 2 \mathrm{D}_c^2 b^{n-2} . c^2 \mp \&c. \\ \mp b^n \pm \mathrm{D} b^{n-1} . bc \mp \mathrm{D}_c^2 b^{n-2} . bc^2 \mp \&c. \end{cases}$

or $\pm b^n \mp \frac{n}{n-1} \mathrm{D} b^{n-1} . c^2 \pm \frac{n}{n-2} \mathrm{D}^2 b^{n-2} . c^2 \mp \frac{n}{n-3} \mathrm{D}^3 b^{n-3} . c^3 \pm \&c.$

$$\left\{ \begin{array}{c} \text{for, since } \mathrm{D}_c^m b^{n-m-1} \times b = \frac{n-2m}{n-m} \mathrm{D}_c^m b^{n-m} \\ \therefore \pm 2 \mathrm{D}_c^m b^{n-m} \mp \mathrm{D}_c^m b^{n-m-1} \times b = \frac{n}{n-m} \mathrm{D}_c^m b^{n-m} \end{array} \right\}$$

Hence, $\alpha^n + \beta^n = b^n - \frac{n}{n-1} \mathrm{D} b^{n-1} + \frac{n}{n-2} \mathrm{D}_c^2 b^{n-2} - \&c.$ c being $= \alpha\beta = 1$.

The law of the series is truly and unambiguously represented, by means of the symbol or note of derivation D; but, if it is required to express the law numerically, in terms of n, since

$$\overset{c}{\mathrm{D}}{}^{m}\, b^{n-m} = \frac{(n-m)\,n-m-1)\,(n-m-2)\,\ldots\ldots\,(n-2m+1)}{1\cdot 2\cdot 3\,\ldots\ldots\ldots\ldots\,m}\, b^{n-2m}$$

$$\alpha^n + \beta^n = b^n - n\,b^{n-2} + \frac{n\cdot n-3}{1\cdot 2}\,b^{n-4} - \frac{n\cdot(n-4)\,(n-5)}{1\cdot 2\cdot 3}\,b^{n-6} + \&c.$$

the series for the chord of the supplement of a multiple arc, in terms of the chord (b) of the supplement of the simple arc.

XVIII. Similar series may be found for the sines and cosines of multiple arcs; thus,

$$\cos.\, z = 2^{-1}\left\{\varepsilon^{z\sqrt{-1}} + \varepsilon^{-z\sqrt{-1}}\right\},\ \cos.\, nz = 2^{-1}\left\{\varepsilon^{nz\sqrt{-1}} + \varepsilon^{-nz\sqrt{-1}}\right\}.$$

Now, $\alpha = \varepsilon^{z\sqrt{-1}}$ $\therefore \alpha^n = \varepsilon^{nz\sqrt{-1}}$. Let $\cos.\, z = p$,

$$\therefore \alpha + \beta = 2p = b,$$

$$\therefore \cos.\, nz = \frac{\alpha^n+\beta^n}{2} = \tfrac{1}{2}\cdot\left(2^n p^n - n\cdot 2^{n-2} p^{n-2} + \frac{n\cdot n-3}{1\cdot 2}\,2^{n-4} p^{n-4} - \&c.\right)$$

$$= 2^{n-1} p^n - n\cdot 2^{n-3} p^{n-2} + \frac{n\cdot n-3}{1\cdot 2}\,2^{n-5} p^{n-4} - \&c. \quad \ldots\ldots$$

$$\text{or} = \tfrac{1}{2}\left\{(2p)^n - n\cdot(2p)^{n-2} + \frac{n\cdot n-3}{1\cdot 2}\,(2p)^{n-4} - \&c.\right\}$$

Suppose it were required to write the series in an inverse order: let n be even, then the series $b^n - \frac{n}{n-1}\,\mathrm{D}\,b^{n-2}$ &c. terminates at a term $\frac{n}{n-m}\,\overset{c}{\mathrm{D}}{}^{m}\, b^{n-m}$, $m = \frac{n}{2}$, and $\therefore \frac{n}{n-m} = 2$, and

$$\alpha^n + \beta^n = \pm 2 \mp \frac{n}{n-m+1}\,\overset{c}{\mathrm{D}}{}^{m-1}\, b^{n-m+1} \pm \frac{n}{n-m+2}\,\overset{c}{\mathrm{D}}{}^{m-2}\, b^{n-m+2} \mp \&c.$$

or, in terms of n,

$$= \pm 2 \mp \frac{n\cdot n}{1\cdot 2\cdot 2}\,b^2 \pm \frac{\left(n\cdot\frac{n}{2}\cdot\frac{n}{2}+1\cdot\frac{n}{2}-1\right)}{1\cdot 2\cdot 3\cdot 4}\,b^4 \mp \&c.$$

Consequently, $\cos.\, nz = \frac{\alpha^n+\beta^n}{2} = \pm 1 \mp \frac{n^2 b^2}{1\cdot 2} \pm \frac{n^2\cdot(n^2-4)\,b^4}{1\cdot 2\cdot 3\cdot 4}$ &c.

Where the upper or lower sign takes place, as n is of the form $4s$, (s an even or odd number), or $2s$, (s an odd number); let n be odd, then the series terminates at a term $\frac{n}{n-m}\,\overset{c}{\mathrm{D}}{}^{m}\, b^{n-m}$, $m = \frac{n-1}{2}$, and $\therefore \frac{n}{n-m}\cdot\overset{c}{\mathrm{D}}{}^{m}\, b^{n-m} = n\,b$,

and $\alpha^n + \beta^n = \pm n\,b \mp \frac{n}{n-m+1}\,\overset{c}{\mathrm{D}}{}^{m-1}\, b^{n-m+1} \pm \&c.$

or in terms of n

$$= \pm n\,b \mp \frac{n\cdot(n^2-1)}{2^2\cdot 1\cdot 2\cdot 3}\,b^3 \pm \frac{n\cdot(n^2-1)\,(n^2-9)}{2^4\cdot 1\cdot 2\cdot 3\cdot 4\cdot 5}\,b^5 \mp \&c.$$

Consequently,

$$\cos.\,nz = \frac{\alpha^n+\beta^n}{2} = \tfrac{1}{2}\Big\{ \pm n\,b \mp \frac{n.(n^2-1)}{1\cdot 2\cdot 3}\frac{b^3}{2^2} \pm \frac{n.(n^2-1)\,(n^2-9)}{1.2.3.4.5}\frac{b^5}{2^4} \Big\}$$

$$= \pm np \mp \frac{n.(n^2-1)}{1\cdot 2\cdot 3}p^3 \pm \frac{n.(n^2-1)\,n^2-9}{1.2.3.4.5}p^5 - \&c.$$

Where the upper or lower sign is to be used, as n is of the form $(4s+1)$, or $4s+3$.

XIX. Again, sine $z = (2\sqrt{-1})^{-1}\left\{ \varepsilon^{z\sqrt{-1}} - \varepsilon^{-z\sqrt{-1}} \right\}$,

sine $nz = \left\{ 2\sqrt{-1} \right\}^{-1}\left\{ \varepsilon^{nz\sqrt{-1}} - \varepsilon^{-nz\sqrt{-1}} \right\}$,

$\therefore$ it is necessary to find $\alpha^n - \beta^n$ in terms of $\alpha - \beta$.

Let n be odd,

then term affected with x^n in developement of $\left\{ \frac{1}{1-\alpha x} + \frac{1}{1+\beta x} \right\}$

$= \alpha^n - \beta^n$, and $\frac{1}{1-\alpha x} + \frac{1}{1+\beta x} = \frac{2-(\alpha-\beta)\,x}{1-(\alpha-\beta)\,x-\alpha\beta x^2} = \frac{2-bx}{1-bx-cx^2}$,

and the term affected with x^n in the developement of $(2-bx)$

$(1-bx-cx^2)^{-1} = b^n + \frac{n}{n-1}\,\mathrm{D}\,b^{n-1}c + \frac{n}{n-2}\,\mathrm{D}^2\,b^{n-2}c^2 - \&c.$

or in terms of n $(c=1)$

$= b^n + n\,b^{n-2} + \frac{n\cdot(n-3)\,(n-4)}{1\cdot 2}\,b^{n-4} - \&c.$

but sine $z = \frac{b}{2\sqrt{-1}} = p \therefore (b)^n = (2\sqrt{-1}\,p)^n = \pm 2^n\sqrt{-1}\,p^n$,

where the upper or lower sign is to be used, according as n is of the form $4s+1$, or $4s+3$. Hence,

$$\text{sine}\,nz = \frac{\alpha^n-\beta^n}{2\sqrt{-1}} = \pm 2^{n-1}p^n \mp 2^{n-3}.np^{n-2} \pm 2^{n-5}.\frac{n.(n-3)\,(n-4)}{1\cdot 2\cdot 3}p^{n-4}$$

&c.

If it is required to write the series in a reverse order, it is to be observed, that the series $b^n + \frac{n}{n-1}\,\mathrm{D}\,b^{n-1}$ &c. terminates at

a term $\frac{n}{n-m}\mathrm{D}_c^m b^{n-m}$, $m=\frac{n-1}{2}$ $\therefore \frac{n}{n-m}\mathrm{D}_c^m b^{n-m}=\frac{nb}{2}$;
consequently,

$\alpha^n-\beta^n=\frac{nb}{2}-\frac{n}{n-m+1}\mathrm{D}_c^{m-1} b^{n-n+1}$ &c.

or in terms of n,

$=\frac{nb}{2}+\frac{n\cdot(n+1)\cdot(n-1)}{1.2.3}\cdot\frac{b^3}{2^2}+\frac{n\cdot(n+1)\cdot(n-1)\cdot(n+3)\,n-3}{1.2.3.4.5}\cdot\frac{b^5}{2^4}$ &c.

Hence, $\frac{\alpha^n-\beta^n}{2\sqrt{-1}}(\text{sine } nz)=p-\frac{n\cdot(n^2-1)}{1.2.3}-p^3+$ &c.

XX. Let n be even, then term affected with x^n in developement of $\left\{\frac{1}{1-\alpha x}-\frac{1}{1+\beta x}\right\}=\alpha^n-\beta^n$.

Now $\frac{1}{1-\alpha x}-\frac{1}{1+\beta x}=\frac{(\alpha+\beta)x}{1-(\alpha-\beta)x-\alpha\beta x^2}$, and the term affected with x^{n-1} in the developement of $(1-bx-cx^2)^{-1}$ is $b^{n-1}+\mathrm{D}b^{n-3}c+\mathrm{D}_c^2 b^{n-5}c^2+\mathrm{D}_c^3 b^{n-7}c^3+$ &c.

$\therefore$ term affected with x^n in $b^1 x\left\{1-bx-cx^2\right\}^{-1}$, $\left\{\alpha+\beta=b^1\right\}$ is $b^1\left\{b^{n-1}+\mathrm{D}b^{n-3}c+\mathrm{D}_c^2 b^{n-5}c^2+\text{&c.}\right\}$

or in terms of n $(c=1)$ is $b^1\left\{b^{n-1}+\left\{n-2\right\}b^{n-3}+\frac{(n-3)\,n-4}{1.2}b^{n-5}-\text{&c.}\right\}$

Hence, since sine $z=\frac{\alpha+\beta}{2\sqrt{-1}}=\frac{b}{2\sqrt{-1}}=p$ $\therefore \frac{b^{n-1}}{\sqrt{-1}}=\pm 2^{n-1}p^{n-1}$, and cosine $z=\frac{\alpha-\beta}{2}=\frac{b^1}{2}=p^1$ $\therefore$ sine $nz=\frac{\alpha^n-\beta^n}{2\sqrt{-1}}$

$=p^n\left\{\pm 2^{n-1}p^{n-1}\mp 2^{n-3}\cdot(n-2)p^{n-3}\pm\frac{2^{n-5}\cdot(n-3)\,n-4}{1.2}\right\}\times$

or $=p^n\left\{\pm(2p)^{n-1}\mp(n-2)(2p)^{n-3}\pm\text{&c.}\right\}$

$\times p^{n-5}\mp$ &c. the upper signs taking place, if n is of the form $2s$ (s odd), the lower, if n is of the form $4s$, s even or odd.

If it is required to write the series in a reverse order, it is to be observed, that the series $b^{n-1}+\mathrm{D}b^{n-3}+$ &c. terminates at a term $\mathrm{D}_c^m b^{n-m-1}$, when $m=\frac{n}{2}-1$; consequently, $\mathrm{D}_c^m b^{n-m-1}=\frac{nb}{2}$, and $\therefore \alpha^n-\beta^n=b^1\left\{\frac{nb}{2}+\mathrm{D}_c^{m-1} b^{n-m}+\text{&c.}\right\}$

$$= b^1\left\{\frac{nb}{2} + \frac{\left(\frac{n}{2}+1\right)\frac{n}{2}\cdot\left(\frac{n}{2}-1\right)}{1.2.3}b^3 + \&c.\right\}$$

$$= b^1\left\{\frac{nb}{2} + \frac{n(n^2-4)}{1.2.4}\cdot\frac{b^3}{2^3} - \&c.\right\}$$

consequently, sine nz or $\frac{\alpha^n-\beta^n}{2\sqrt{-1}}$

$$= p^1\left\{np - \frac{n\cdot(n^2-4)}{1.2.3}p^3 + \frac{n\cdot(n^2-4)\,n^2-9)}{1.2.3.4.5}p^5 - \&c.\right\}$$

XXI. The sine nz (n even) may be expressed by series, in terms of the cosine of z;

thus, $\frac{1}{1-\alpha x} - \frac{1}{1-\beta x} = \frac{\left\{\alpha-\beta\right\}x}{1-\alpha+\beta x+cx^2}$,

and, equating the terms affected with x^n in each developement, we shall have

$$\text{sin. } nz = p\left\{(2p^1)^{n-1} - \frac{n-2}{1}(2p^1)^{n-3} + \frac{(n-3)(n-4)}{1.2}(2p^1)^{n-5} - \&c.\right\}$$

when n is even, a series may be found for sin. nz in terms of p (sin. z) only; but this series will not terminate as all the foregoing series do.

To find this series, expand $\sqrt{(1-p^2)} = p^1$ into a series,

$$1^{\frac{1}{2}} - D1^{\frac{1}{2}}p^2 + \underset{c}{D^2}\,1^{\frac{1}{2}}p^4 - \&c.$$

then sin. $nz = \left\{1 - D1^{\frac{1}{2}}p^2 + \underset{c}{D^2}\,1^{\frac{1}{2}}p^4 - \&c.\right\} \times \left\{np - \frac{n.(n^2-2^2)}{1.2.3.}p^3 + \&c.\right\}$

$$= np + Ap^3 + A_{,}p^5 + A_{,,}p^7 + \&c.$$

in which series, the law of the coefficients, or a general expression for $A_{,\ldots,n}$ may be found. But it cannot now be done, without too long a digression from the present objects of inquiry.

From what has been done, the series* of the chord of the

* Demonstrations of these forms have been given by reversion of series, and by induction; which demonstrations are imperfect, since they do not exhibit the general law of the coefficients. See De Moivre Miscell. analytica. Epistola de Cotesii Inventis, &c. Newtoni Opera omnia, p. 306. Euler in Analyt. inf. Cap. 14. Waring has deduced the chord of the supplement of a multiple arc, in terms of the chord of the supplement of the simple arc, from his theorem for the powers of roots:

multiple arc may be found in terms of the chord of the simple arc; for, chord $nz = \left\{\sqrt{-1}\right\}\left\{\epsilon^{\frac{nz}{2}\sqrt{-1}} - \epsilon^{-\frac{nz}{2}\sqrt{-1}}\right\}$.

XXII. In the above demonstrations, no formulas are borrowed from geometry; and the general law of the coefficients is clearly expressed; it is, I think, most conveniently expressed by means of the symbol or note of derivation D. The operation which this symbol indicates is as certain as any other operation, whether arithmetical or algebraical.

XXIII. The demonstrations and method of deduction given in this paper shew, I think, with sufficient evidence, the introduction of geometrical expressions and formulas into analytical investigation to be perfectly unnecessary. It has appeared likewise, that such introduction embarrasses investigation, and causes ambiguity, by concealing the true derivation of expressions; and, although I do not wish to give importance to my own observations, by supposing a greater confusion of notion to exist than really does, yet, I think, in what has been written and said, there may be detected a lurking opinion, that the value of certain expressions essentially demand the existence of geometrical curves and figures, and the investigation of their properties.

XXIV. In the Appendix to the *Arithmetica Universalis*, p. 200. 219. &c. NEWTON, with great clearness and force of argument, has shewn the distinction to be made between the order of classing curves, analytically considered, that is, defined

but the demonstration of the latter theorem is not, it appears to me, to be reckoned in the number of strict demonstrations. The only objection against the demonstration of the very learned and ingenious author of the Calcul des Derivations, is, that it is rather indirect, and blended with geometrical expressions and formulas.

by equations, and the order of classing them, considered as generated by description. Moreover, he animadverts on the custom of confounding the two sciences of algebra and geometry;* and, if any authority is attached to his assertion, that the two sciences ought not to be confounded together, the separation of geometry from algebra will thereby be equally urged as the separation of algebra from geometry. And it cannot be said with greater truth, that the simplicity of geometry is vitiated with algebraic equations, than that the simplicity of analysis is vitiated with geometrical forms and expressions. In fact, each science ought to be kept distinct; and be made to derive its riches from its proper sources.

XXV. It will not demand much meditation to be assured of this truth, that, in any mathematical investigation, the geometrical method, properly so called, is not essentially or absolutely necessary. The properties of extension and figure, to which this method has been especially appropriated, may be analytically treated; and here it is proper to state a distinction necessary to be made, between what may be called analytical geometry, and the application of analysis to geometry. The first does not suppose or require the existence of such a method as the geometrical; but, from a few fundamental principles, analytically investigates the properties of extension; whereas, in the latter, analysis is applied to propositions already established by the geometrical method: so that, strictly, to shew the justness and propriety of the application, a separate investigation is

* "Multiplicationes, divisiones, et ejusmodi computa, in geometriam recens introducta sunt: idque inconsulto, et contra primum institutum scientiæ hujus, &c. —— Proinde hæ duæ scientiæ confundi non debent, &c.—— Et recentes, utramque confundendo, amiserunt simplicitatem in quâ geometriæ elegantia omnis consistit."

necessary. We find, however, in general, a vague analogy substituted, as a connecting principle between the two methods.

XXVI. The application of algebra to geometry, gives to DESCARTES the fairest title to fame for mathematical invention; yet the cause and nature of the benefit conferred on science by that application, seems to be indistinctly apprehended.* For, the Analytical Calculus, when applied to geometry, was not enriched with the truths of the latter science, because some connecting principle had been discovered, or some process invented by which the property of the two methods became common, and might, from one to the other, without formality be transferred; but because the investigation of certain properties could not proceed, without first improving the means by which they were to be investigated. These means DESCARTES improved: he found, when certain conditions in problems concerning extension were translated into the language of algebra, that the process of deduction with the general terms was slow and incommodious, because, such was the low state of the algebraic Calculus, the relation between the general terms had not been established. The aim and merit of DESCARTES's speculations is to have established this relation. If illustration were needed to make my meaning clear, I should say that DESCARTES, NEWTON, and D'ALEMBERT, benefited science precisely after the same manner. The first applied the analytical Calculus to extension; the second to motion; the third to the equilibrium, resistance, &c. of fluids. As the object of investigation became

* Thus far was the Analytical Calculus benefited by the existence of the geometrical method: certain properties of figure and extension, discovered by the latter, became to the former, objects of investigation.

more abstruse, it was found necessary to improve more and more the means or instrument of investigation.

XXVII. As the question concerning the respective advantages of the ancient geometry and modern analysis, is not foreign to the subject of this Paper, I shall briefly state it, and endeavour to afford the means of arriving therein at something like a precise determination.

The superiority of one method above another, must consist in being either more logically strict in its deductions, or more luminous, or more commodious for investigation. The discussion concerning the strictness and accuracy may, I conceive, be immediately put aside, since no method of deduction is essentially inaccurate; and, if in geometry the inferences are more strictly deduced than in the algebraic Calculus, the advantage is to be reckoned an accidental one, and arising from the great attention with which the former science has been cultivated.

One method may, however, be essentially more perspicuous and more commodious for investigation than another; or, in other words, the perspicuity and commodiousness of a method may depend on circumstances inherent in its nature and plan. Now, a person not sensible of the superior perspicuity of the geometrical method, would demand these circumstances, the necessary causes of perspicuity, to be pointed out to him; which might be done, by stating that geometry, instead of a generic term, employs, as a particular individual, the sign or representative of a genus; and that, as in algebra, the signs are altogether arbitrary, in geometry, they bear a resemblance to the things signified, and are called *natural* signs, since the figure of a triangle, or square, suggests to the mind the same tangible figure in Europe, that it does in America: and this resemblance

of the sign to the thing signified, is supposed to be the chief cause of the superior clearness of geometrical demonstration.* Another cause may perhaps be thought to exist in this circumstance, that whatever is demonstrated, of a triangle or other diagram, considered as the representative of all triangles and diagrams, is moreover demonstrated of that individual triangle or diagram. A third, and more satisfactory cause than the last, may be, that in investigation, for the purpose of preventing ambiguity and mistake, it is frequently necessary to recur from the sign to the thing signified; which is more easily done, the less general and arbitrary the modes of representation are; and, consequently, in geometry more easily than in algebra.

I do not pretend to have assigned, accurately, and all, the causes of perspicuity of geometrical reasoning. It may depend on certain intellectual acts and processes, which it is beyond the power of philosophy to explain. The circumstance, however, of the signs employed in geometry being *natural* signs, will prove its perspicuity only to a certain extent, and in certain cases. It must fail to prove it, when the properties of solids are treated geometrically; because the representation of solids on a plane by diagrams, is not a natural representation, that is, would not suggest to all minds the same tangible portion of extension.

It must fail likewise to prove it, in questions concerning radii of curvature, areas of curves, &c. or in all questions in which the fluxionary or differential Calculus is usually employed. The

* Does there not, however, here arise a consideration that takes away from the cause of the perspicuity of geometrical demonstration? For the reasoning with a diagram cannot be generally true, except the diagram be considered abstractedly, and independent of those peculiar and distinguishing properties that determine its individuality.

lines and mixtilinear triangles therein exhibited cannot be called natural signs, since they are only imperfect and inadequate representations of other imaginary lines and triangles, of which the mind must form what notion it can. Not, however, to infer want of perspicuity from inefficiency in the cause assigned, if we employ the geometrical method, or view its employment in investigation, concerning motion, curves, &c. it will not appear a perspicuous method; and, if instances of its obscurity were required of me, I could find them, even in the immortal work of the Principia. Whether we consider the fact, or speculate about the cause, I think the geometrical method can only be allowed to have superior evidence in investigations of a simple nature.

That the analytical calculus is more commodious for the deduction of truth than the geometrical, will not perhaps be contested; and, an examination into its nature, would shew why it is so well adapted for easy combination and extensive generalization. No language like the language of analysis, one of the greatest of modern mathematicians has observed, is capable of such elegance as flows from the developement of a long series of expressions connected one with the other, and all dependent on the same fundamental idea.

If we view what has been respectively done by each method, in the explanation of natural phenomena, the superiority of the one above the other will appear immense: yet the cultivators of geometry were men of consummate abilities, and possessed this great advantage, that the method or instrument of thought and reasoning which they employed had, during preceding times, received the greatest improvement. The analytical calculus, which has verified the principle of gravitation, was a hundred years ago in its infancy.

The question, then, concerning the respective advantages of the ancient geometry and modern analysis, may be comprised within a short compass. If mental discipline and recreation are sought for, they may be found in both methods; neither is essentially inaccurate; and, although in simple inquiries the geometrical has greater evidence, in abstruse and intricate investigation the analytical is most luminous: but, if the expeditious deduction of truth is the object, then I conceive the analytical calculus ought to be preferred. To arrive at a certain end, we should surely use the simplest means; and there is, I think, little to praise or emulate, in the labours of those who resolutely seek truth through the most difficult paths, who love what is arduous because it is arduous, and in subjects naturally difficult toil with instruments the most incommodious.

XXVIII. If in matters of abstract science deference is ever due to authority, it must be paid to that by which the study and use of the method of the ancients has been recommended. NEWTON has, however, brought forward no precise arguments in favour of synthesis; and it is easy to conceive, that he would be naturally attached to a method long known and familiar to him,* and by means of which he was enabled to connect his own theory of curvilinear motions, with the researches of the ancients on conic sections, and with HUYGENS's discoveries relative to central forces and the evolutes of curves.

The very ingenious and learned MATTHEW STEWART† endea-

* The circumstance of mathematicians having acquired a considerable dexterity in the management of the geometrical method, seems to be the reason why they endeavoured to explain the doctrine of logarithms (a subject purely algebraical) by the introduction of the properties of curves.

† Words are frequently stated in a delusive and imposing manner, not always

voured to shew, that the geometrical calculus was competent to the explanation of natural phenomena; and with astonishing perseverance applied it to many investigations in physical astronomy. The labours of such a man are not hastily to be judged: yet every one must determine for himself; and to me it seems, his reasonings, from their intricacy, call up so great a *contention of the mind*, that they prove, in no small degree, the unfitness of the geometrical method in all abstruse and intricate investigations.

XXIX. It may, however, be asked, are not there some subjects of inquiry to which the geometrical method is better adapted than the analytical? and is not the theory of angular functions one of these subjects?* I apprehend not: for, if the conditions

intentionally. Dr. STEWART, (Preface to Sun's Distance,) and after him his ingenious biographer, for the purpose of holding up the superior simplicity of the geometrical calculus, has said, that in order to understand his solution, a knowledge of the elements and conic sections only is requisite. But, in fact, the solution is effected by proposition heaped on proposition; and with equal truth and justness it might be said, that in order to understand the analytical solution, a knowledge only of common algebra is requisite; since the methods by which the solution is effected, are really and properly branches of algebra.

* D'ALEMBERT says, "there are cases in which analysis, instead of expediting, embarasses demonstration. These cases happen in the computation of angles: for angles can analytically be expressed only by their sines; and the expression of the sines of angles is often very complicated," &c. He adds, "that it must depend on mathematicians, whether the method of the ancients or the modern analysis is to be employed, since it would be difficult to give on this head exact and general rules." In the very case adduced, I think demonstration expedited by the analytical calculus; and, although $2^{-1}\left\{e^{x\sqrt{-1}}+e^{-x\sqrt{-1}}\right\}$ is not so speedily put down as cos. x; yet all processes of evolution, differentiation, integration, &c. are much more easily performed with the former expression than with the latter. Other instances of subjects of inquiry, to which the geometrical method is said to be peculiarly well adapted, have been adduced; but I still find no convincing reason, why a mathematician must submit to the necessity of

can be adequately and unambiguously stated in the general terms of algebra, then deduction with such terms may be strictly made, and expeditiously; since it is to be made according to a known and established process. I have shewn at some length, that reasoning may be conducted with terms which separately cannot be arithmetically computed: for the mere process of deduction, it is not necessary to have distinct and complete notions of the things signified by the general terms.

The principal object of the present paper is to shew, that the analytical calculus needs no aid from geometry, and ought to reject it, relying entirely on its own proper resources. By this means, it would gain perspicuity, precision, and conciseness; advantages not to be lightly estimated, by any one who has a regard to certainty and demonstration, or considers the bulk to which scientific treatises have of late years swelled.

In order to prove and illustrate the opinion I wished to establish, I directed my search to those cases which have been always thought to require the aid of the geometrical method. By a purely analytical process, I have traced the origin and derivation of certain fluxionary expressions, usually referred to logarithms and circular arcs. I have given demonstrations of the series for the sine of an arc in terms of the arc; of the analytical formula for the root of a cubic equation in the irreducible case; of the resolution of $x^n \mp a^n$ into quadratic factors; of the series for the chord, &c. of a multiple arc in terms of the simple arc, &c. which demonstrations, with as much confi-

learning half a series of truths by one method, and half by another. These considerations, however, depreciate the value of the geometrical method only in one point of view; for, after all, the finest exemplar of clear and accurate reasoning is contained in the works of EUCLID.

dence as I dare assume, knowing how fallaciously we judge of our own performances, I affirm to be strict and direct; established without artifices, and without foreign aid drawn from geometrical theorems and the properties of curves. In some parts of this paper, the subjects, for their importance, may be thought to be too slightly discussed; the fear of appearing prolix, has perhaps driven me into brevity and obscurity. In other parts, what I have advanced may be remote from common apprehension, or contrary to received opinion: but here I make no apology; for, what I have written, has been written only after long meditation, and from no love of singularity: "If I cannot add to truth," I do not desire distinction from "the "heresies of paradox."

VI. *Observations and Experiments upon oxygenized* and hyperoxygenized muriatic Acid; and upon some Combinations of the muriatic Acid in its three States.* By Richard Chenevix, *Esq. F. R. S. and M. R. I. A.*

Read January 28, 1802.

When Mr. Bertholet made known the combination of what was then called oxygenated muriatic acid with potash, he gave as his opinion, that the proportion of oxygen, relatively to the quantity of acid, was greater in the salt than in uncombined oxygenized muriatic acid. This conjecture was fairly founded upon the observation, that, in his mode of preparing this salt, a large portion of common muriate was formed in the liquor, along with the hyperoxygenized muriate. The Memoir which he published in the year 1788, is the last with which I am acquainted, upon this subject. It does not contain any thing that, considering the accuracy which is now required in experiments, amounts to a demonstration of the relative proportions of oxygen, in oxygenized and hyperoxygenized muriatic acids. Unfortunately, this chemist has not pursued his researches any farther; although, from his own words, we had every reason to hope that they would have been continued.

In the *Système des Connoissances chimiques* of Mr. Fourcroy,

* I have preferred this word to *oxygenated*, because *ate* is the appropriate termination of certain salts formed by the acids in *ic*. Some further remarks upon this subject will be made in a work now in the press, entitled *Remarks upon Chemical Nomenclature*.

we find a summary of the experiments that had preceded the impression of his work, together with the following sentence. "Tous les muriates suroxygenés sont décomposés par les acides, "souvent avec une violente décrepitation, avec une dégagement "de vapeur jaune verdâtre, et une odeur très-forte. Cette vapeur "est de véritable acide muriatique suroxygéné. Elle est lourde, "tombe en goutellettes d'un jaune vert, et forme des stries "comme de l'huile, sur les corps auxquels elle adhère." This assertion carries no confirmation along with it; and does not amount so near to proof as the position of the former chemist: so that, in fact, the existence of hyperoxygenized muriatic acid, and of its combination with potash, rests, at present, upon the conjecture of Mr. BERTHOLLET; a conjecture however which, as well as his whole dissertation upon the subject, bears all the marks of genius which so strongly characterise every production of that sagacious philosopher. Some notice has been taken of other saline combinations, formed by causing a current of oxygenized muriatic acid to pass through solutions of the alkalis, or earths, or by otherwise combining them. Mess. D'OLFUS, GADOLIN, VAN-MONS, LAVOISIER, and others, have slightly mentioned some of these combinations. But, with the exception of Mr. BERTHOLLET, I know of no chemist who has approached so near to the real state of the combination of muriatic acid and oxygen with potash, as Mr. HOYLE, of Manchester. The true nature of this salt, however, is one of those things which many persons have credited without proof; and which many others have been on the eve of discovering.

I shall now proceed to lay before the Society, an account of the observations and experiments which have led me to conclude, that muriatic acid does exist in the form of oxygenized

and hyperoxygenized muriatic acid, as announced in the title of the present communication; and that, in either state, it is capable of entering into saline combinations.

With this view, I shall describe,

1st. The means by which I think I have succeeded, in ascertaining the constituent parts, as well as the proportions, in oxygenized and hyperoxygenized muriatic acid.

2dly. I shall mention some of the combinations of the muriatic acid, in its three states.

In treating upon the first of these objects, I must in some measure anticipate the second; and must suppose some things known, which are hereafter to be described. This inconvenience is inevitable; as the natural order of things leads me to treat of the acid, before I consider the bodies into the composition of which it enters.

I exposed to the heat of a lamp, 100 grains of hyperoxygenized muriate of potash. It decrepitated gently, and in a short time melted. After remaining in fusion nearly an hour, I allowed it to cool: it crystallized as formerly, and had lost 2,5 per cent.. I increased the heat to redness, in a furnace. The salt boiled with a violent effervescence, and rapid disengagement of gaseous fluid, together with a thin white vapour, and then sunk suddenly into a white spongy mass. The loss of weight usually varied from 42 to 48 or 50 per cent.

I put 100 grains into a coated glass retort, luted to a small and perfectly dry receiver, having a tube communicating with a glass bell in the pneumatic tub. The fire had not been lighted very long, when a slight dew began to line the inside of the receiver; and, as soon as the retort was nearly red hot, a disengagement of gas, so sudden as almost to be explosive, took

place. A quantity of thin white vapour arose, which afterwards was deposited, in the form of a white sublimate, in the receiver and the tube. When the extrication of gas had ceased, the apparatus was allowed to cool. The gas, with the usual corrections of temperature and pressure, measured 112,5 cubic inches, = 38,3 grains. The 2,5 mentioned above, as the loss of this salt at a low heat, were water. 53,5 remained in the retort; and the white sublimate in the tube and receiver amounted to 5.

The products of this operation were therefore,

Water - - - -	2,5
Oxygen - - - -	38,3
Salt in the tube and receiver -	5
Salt remaining in the retort - -	53,5
	99,3.

To find the proportions of oxygen and muriatic acid, in hyperoxygenized muriatic acid, it now only remains to determine the sum of the quantities of muriatic acid, contained in the 53,5 of the retort and the 5 of the tube and receiver. The 53,5 gave, by nitrate of silver, a precipitate corresponding to 18,21; and the 5, a precipitate corresponding to 1,76; in all, 20 of muriatic acid. Therefore, 38,3 of oxygen, and 20 of muriatic acid, combine to form 58,3 of hyperoxygenized muriatic acid; or, 100 of hyperoxygenized muriatic acid contain, within a fraction,

Oxygen - - - -	65
Muriatic acid - - - -	35
	100.

And the elements of hyperoxygenized muriate of potash, should be thus stated:

Oxygen - 38,3	{ hyperoxygenized	} - -	58,3
Muriatic acid 20	{ muriatic acid	}	
Potash - - -			39,2
Water - - -			2,5
			100,0.

It may be observed, that the 53.5 of the retort did not yield the same proportion of acid as the 5 of the tube and receiver. The fact is, that all muriates lose a little of their acid at a red heat, as I shall presently mention more particularly; and the small loss was, in all probability, owing to a portion of acid disengaged by the heat to which the salt was necessarily exposed during the operation.

Having thus ascertained the proportion of oxygen in hyperoxygenized muriatic acid, by means of its combination with potash, a ready method occurred to arrive at the knowledge of that contained in oxygenized muriatic acid. For this purpose, I disposed in the following manner, a WOULFE's apparatus, consisting of three bottles, and connected with the pneumatic tub. In the first bottle, I put a solution of potash,* in about six parts of water. In the second, a solution of the same; but so dilute, as that no part of the salt, which might be formed, should crystallize during the operation. About twenty parts of water was the proportion there employed. In the third bottle, I put common carbonate of potash. Through this apparatus, I sent a current of oxygenized muriatic acid, disengaged by sulphuric acid, from a mixture of muriate of soda and black oxide of manganese, in the well known manner. Crystals of hyperoxygenized muriate of potash were formed in the liquor of the first bottle; and, as long as they remained, I was certain, from previous experiment, that no sulphuric or muriatic acid could pass into the second bottle. The current was continued, till the liquor of that bottle contained an excess of acid. The carbonate of potash, in the third bottle, absorbed the superabundant vapours; and the

* Whenever potash, soda, barytes, an acid, an alkali, water, or the names of other substances are used without an epithet, they are meant to denote them in that state which is commonly called *pure*.

pneumatic apparatus was ready to collect any gases that might be evolved. By these means, I obtained, in the second bottle, a solution of whatever substance might result from the action of potash upon hyperoxygenized muriatic acid.

I took a portion of this liquor, which I shall call *entire liquor*,* and distilled it to dryness in a glass retort, taking care to screen it from light. A tube from the receiver communicated with the pneumatic tub. My object was to ascertain, whether the change observed by Mr. BERTHOLLET, in the distribution of the elements of oxygenized muriatic acid, to form, with potash, a simple and a hyperoxygenized muriate, really took place among those elements themselves, independently of any absorption of oxygen from the atmosphere, or extrication of it from the salt. Nothing but some water, and a few inches of the dilated air of the vessels, passed into the receiver and the pneumatic apparatus; and I found, in the retort, a saline mass,† perfectly dry and crystallized. Hence it is evident, that the same quantity of oxygen as that formerly contained in the oxygenized muriatic acid, which had been united to the alkali, to form the total mass of salt, was now condensed, in that part which had become hyperoxygenized muriate.

To ascertain this quantity, I dissolved 100 grains of the entire salt in water, and precipitated by nitrate of silver. I thus obtained a quantity of muriate of silver, which, by proportions previously determined, I knew to correspond to 84 of muriate

* I am well aware that, upon philosophical principles, this appellation is objectionable; but, for the sake of brevity, I have used it as a temporary name, for a substance which has only a relative existence among chemical bodies.

† This salt, I shall call *entire salt*.

of potash: therefore, 16 were hyperoxygenized muriate of potash.* But, according to the proportions established above in hyperoxygenized muriate of potash, 16 of this salt contain 6 of oxygen, with 3,20 of acid, the remainder being alkali and water; and, by preliminary experiments, I found that 84 of muriate of potash contained 27,88 of muriatic acid. Therefore, 27,88 + 3,20 = 31,08 of muriatic acid, with 6 of oxygen, or, to reduce it to the quintal,

Muriatic acid - - -	84
Oxygen - - - -	16
	100, are the proportions

which combine to form oxygenized muriatic acid.

To corroborate this evidence, I distilled 100 grains of the entire salt mentioned above; and obtained nearly 16,5 cubic inches of oxygen gas; which as accurately corresponds with the trial by nitrate of silver, as can be expected in experiments of this nature.

Mr. BERTHOLLET, in his Memoir upon oxygenized muriatic acid, gives, if I understand him rightly, the following statement of the proportions, and of the means by which he obtained his results. He exposed to the light of the sun, 50 cubic inches of water, saturated with oxygenized muriatic acid; and collected in the pneumatic tub, 15 cubic inches of oxygen gas. I here neglect fractions; because our results appear, at first sight, to differ so widely as not to require great accuracy in giving their comparative statement. He then preci-

* I must observe here, that hyperoxygenised muriate of potash does not, like simple muriate, decompose the salts of silver. This shall be further animadverted upon, and proved, in its proper place.

pitated, by nitrate of silver, the 50 cubic inches of liquor, which had become simple muriatic acid, and obtained 38,3 grains of muriate of silver. But, by experiments, I found that 38,3 of muriate of silver contain 6,5 of muriatic acid. Therefore, 6,5 of muriatic acid combine with 15 cubic inches* (=8 grains) of oxygen, to form 73 of oxygenized muriatic acid. But 73 : 8 :: 100 : 11, or nearly. For this difference, however, it may be easy to account. Perhaps Mr. BERTHOLLET's 50 cubic inches of oxygenized muriatic acid, contained originally a little simple muriatic acid; and he says besides, that he suspects all the oxygen was not disengaged. This indeed is most probable; and I am happy that I can reconcile the proportions which I have found, to the opinion of so skilful a chemist.

Mr. CRUIKSHANK likewise, in his additional Observations upon Hydrocarbonates, has stated that 2,3 parts of oxygenized muriatic acid contain 1 of oxygen, or about 43,5 per cent. But this able chemist, to whom we are indebted for the discovery of the gaseous oxide of carbone, procured his oxygenized muriatic acid by a peculiar method, which I shall notice, in speaking of the action of acids upon hyperoxygenized muriate of potash. The substance he obtained was, in fact, not oxygenized muriatic acid gas, but a mixture of that gas with hyperoxygenized muriatic acid. I have not the smallest doubt of the accuracy of his statement; but, being the proportion of a mixture, it in no way contradicts either of those I have determined in this Paper.

Before I dismiss this part of the subject, I wish to anticipate an objection, founded upon an observation of Mr. BERTHOLLET, which may be made to the above experiments. He says, that when the alkaline solution is very concentrate, an effervescence

* Mr. BERTHOLLET's proportions are in the old French weights and measures.

takes place during the whole of the saturation, and for some days after; and this effervescence, he attributes to the escape of oxygen. But I have already said, that no oxygen gas was disengaged in any part of my process; and no effervescence took place in any of the bottles, except the third; so that, no superabundance of oxygen could have passed from one into the other, nor could any diminution of the total quantity have been produced. By repeating the experiments, sometimes with a solution of alkali, and sometimes with water alone, in the first bottle, I obtained the liquor in the second bottle uniform in all cases. Indeed, as potash prepared in Mr. BERTHOLLET's manner, was not in such general use at the time he performed his experiments as at present, I suspect that a great part of this effervescence was owing to a disengagement of carbonic acid from the alkali.

Having thus proved the difference between the states of these two acids, I shall now proceed to the combination of each with salifiable bases.

OXYGENIZED MURIATES.

As many properties of the entire liquor, before it had been evaporated to dryness, had led me to imagine that the acid was united with the alkali, and remained in combination with it, in the state of oxygenized muriatic acid, till the moment of crystallization, I think it necessary to state at length the appearances which induced me to draw that conclusion, and the experiments which afterwards convinced me that it was erroneous.

A few drops of sulphuric acid, poured into some of the entire liquor, caused an effervescence, and a smell of hyperoxygenized muriatic acid.

Very strong acetic acid produced the same effect.

By other experiments, I had ascertained that acetic acid could not decompose any part of the entire salt; and hence I concluded, that in the entire liquor, before evaporation, some of the salt remained in the state of oxygenized muriate, the acid of which was expelled by the sulphuric or acetic acid; and, that it was not till the moment of crystallization, that the elements of the salt underwent a total resolution into muriate, and hyperoxygenized muriate, of potash. However, a small quantity of any of the very soluble neutral salts, such as nitrate or muriate of ammonia, or even a little alcohol, produced the same effects; and I was then convinced, that the effervescence was owing to some uncombined oxygenized muriatic acid gas, remaining in the liquor; and which was disengaged, in proportion as the water was taken from it, by the superior affinity of the salt, or the alcohol, I had used.

By some previous experiments, I had ascertained, as I have just mentioned, that acetic or acetous acids do not decompose hyperoxygenized muriate of potash. I sent a current of oxygenized muriatic acid through a solution of acetite of potash; and, upon evaporation, I found that the acetous acid had been disengaged, and that muriate, with hyperoxygenized muriate, of potash had been formed. But, from some trials, which I shall presently relate, I was induced to believe, that oxygenized muriatic acid attracts the salifiable bases with a much weaker affinity than acetous acid. It is well known that the contact of oxygenized muriatic acid with an alkali, is sufficient to produce a combination of that acid with the alkali; and, from the last-mentioned experiments it appears, that it is not absolutely necessary that

the alkali should be in a free state. If it be combined with an acid weaker than hyperoxygenized muriatic acid, the original acid will be expelled; and muriate and hyperoxygenized muriate will be formed, as if the alkali had been free.

As a further proof, that the change in the distribution of oxygenized muriate of potash takes place at the moment of contact of the acid and the alkali, and consequently long before the crystallization, I mention the following experiments.

I precipitated, by nitrate of silver, 400 grains of the entire liquor, previously to its being evaporated; and obtained 71 grains of muriate of silver.

I evaporated to dryness, 400 grains of the same liquor, redissolved the residuum, and, by dropping in nitrate of silver, obtained 70 grains of muriate. The difference of one grain, in these experiments, does not amount to 0,2 of a grain of muriate of silver; and ought not to be regarded.

From these experiments, it is past all doubt, that the original entire liquor did not contain oxygenized muriate of potash. For, if such a combination had existed in it, I should have obtained a smaller portion of muriate of silver in the first than in the second case, on account of the total separation into muriate and hyperoxygenized muriate having not yet taken place.

We are not however to conclude, from these experiments, that there are no such things as oxygenized muriates. Although they cannot be exhibited in a palpable state, it is easy to demonstrate that they do really exist. I shall prove, in the proper place, that hyperoxygenized muriate of ammonia is not an incompatible combination; and must, for the present, assume the datum, in order that I may demonstrate the necessary

existence of oxygenized muriates. Therefore: If muriatic acid, or if hyperoxygenized muriatic acid, be brought in contact with ammonia, the result will be muriate, or hyperoxygenized muriate, of ammonia. But, if the acid, disengaged by sulphuric acid, from a mixture of black oxide of manganese and muriate of soda, be sent through ammonia, both are decomposed. Hence it is evident, that the acid combines with the alkalis, in the state of oxygenized muriatic acid; and that the separation into muriate and hyperoxygenized muriate, is produced by a subsequent action, among the elements of oxygenized muriate of potash.

Upon the whole, it appears to me fair to conclude,

1st. That the salts of this genus do really exist, previous to the formation of hyperoxygenized muriate of potash.

2d. That the affinity exercised by hyperoxygenized muriatic acid for ammonia, and (by very strong analogy) for the other bases, is much greater than that of oxygenized muriatic acid. For, hyperoxygenized muriatic acid, as shall presently be shewn, having a much more powerful action upon all combustible bodies, whether simple or compound, than oxygenized muriatic acid, it would be natural to suppose that the former acid would act more powerfully upon the inflammable element of ammonia. But oxygenized muriatic acid combines with the hydrogen of that alkali; which, however, is not decomposed by hyperoxygenized muriatic acid; yet the affinity of hyperoxygenized muriatic acid for ammonia, is the only cause that determines the union of the acid and the alkali, without decomposition. But these affinities shall be more fully treated of, in speaking of hyperoxygenized muriate of ammonia.

ALKALINE AND EARTHY HYPEROXYGENIZED MURIATES.

Generic Characters.

Hyperoxygenized muriates are formed by passing a current of oxygenized muriatic acid through the basis, dissolved or suspended in water, as in the formation of the last mentioned genus. Their first formation is owing to the separation of the elements of an oxygenized muriate, into hyperoxygenized muriate and simple muriate; from which latter, they may be separated by crystallization, or by another process, which I shall mention, in treating of the earthy hyperoxygenized muriates. By simple trituration, they scintillate, with noise. They are decomposed by a low red heat; and give out a considerable quantity of oxygen, as they become simple muriates. They cannot be brought down, by any means that I have tried, to that diminished state of oxygenizement, which would constitute oxygenized muriates. They inflame all combustible bodies with violence, as is well known. They are soluble in water; many of them, in alcohol; and some are deliquescent. The acid is expelled, with particular phenomena, by sulphuric, nitric, and muriatic acids, without heat; and, a little below a boiling heat, by phosphoric, oxalic, tartareous, citric, and arsenic acids: but they are not acted upon by benzoic, acetic, acetous, boracic, prussic, or carbonic acids. Those vegetable acids which are powerful enough to decompose them, give out, towards the end, a gas of a peculiar nature, which has not so much smell as oxygenized muriatic acid gas, but which affects the eyes in an extraordinary manner, and promotes an uncommon and rather painful secretion of tears. I have not yet examined this gas, as there was invariably an inflammation of the mixture, with explosion and rupture of the

vessels, almost as soon as it began to be evolved. When pure, the hyperoxygenized muriates do not precipitate any of the metallic salts, although I believe they decompose some. The order in which the bases seem to be attracted by the acid, is, potash, soda, barytes, strontia, lime, ammonia, magnesia, alumina, silica. The other earths I have not tried, and but few of the metallic oxides.

1st Species. Hyperoxygenized Muriate of Potash.

This salt is the best known of all the saline combinations of this acid. It has been erroneously considered as simply oxygenized, for its acid is really hyperoxygenized. It is soluble in about sixteen parts of cold water, but in much less of warm; and is easily separated, by crystallization, from muriate of potash. Alcohol can dissolve a small portion of it. It seems capable of existing in more states than one; for, in passing a current of oxygenized muriatic acid, very slowly, and in the dark, through a solution of potash, till saturated, I have obtained flexible and shining needle-like crystals. This leads me to suspect, either a hyperoxygenized muriate of potash with excess of acid, or that acid with a superaddition of oxygen. It would be superfluous to enter into a minute description of a substance so well known as hyperoxygenized muriate of potash; but, it being the substance whence I have chiefly attempted to disengage the acid, I shall enter into a particular detail of the action of the more powerful acids upon this salt.

If concentrate sulphuric acid be poured upon hyperoxygenized muriate of potash, a violent decrepitation, sometimes but rarely accompanied by a flash, takes place. A thick heavy vapour, of a greenish yellow colour, which rises with difficulty to the top of the vessel, if it be deep, is disengaged. The smell is not altogether

unlike nitrous gas; but is peculiarly fetid, and may be compared to that which is emitted by brick-kilns, mixed with that of nitrous gas. It differs much from oxygenized muriatic acid gas; the latter being pungent and penetrating, the other heavy and oppressive; and it does not produce, at least in so great a degree, the catarrhal symptoms caused by the other. At the bottom of this vapour is a bright orange-coloured liquor, which has the same smell as the vapour. This is the acid contained in the salt; and I have proved it to be hyperoxygenized muriatic acid. But, although the salt from which the acid is disengaged be pure, the acid itself is never so; because the very act of disengaging it effects its decomposition, and some of it is converted into oxygenized muriatic acid. The colour of litmus paper, on this account, is generally destroyed by the liquor. I say on this account, because I have some reason to believe, from having observed this not to be uniformly the case, that hyperoxygenized muriatic acid reddens the vegetable blues. However, it must be considered, that the sulphuric acid used to disengage the hyperoxygenized muriatic acid is still present; and we can draw no certain conclusion, until we have obtained this acid free from all other substances. If to this mixture of hyperoxygenized muriate of potash and sulphuric acid, heat be applied, an exceedingly violent explosion, with a white and vivid flash, takes place, before the liquor has attained the temperature of 195 of FAHRENHEIT. In order to obtain this acid, I attempted to distil 500 grains, in a glass retort, in a water bath, with every precaution against such accidents as I could not but in some measure expect; when, almost as soon as I had kindled the fire, I saw, in the bottom of the retort, an extremely white, vivid, and rapid flash, which was immediately followed by a loud report. The retort was reduced almost to

powder; so that scarcely any fragments of it could be found in the laboratory. The windows, and several glass vessels, were broken. I happened to be holding the neck of the retort, at the moment of the explosion, yet received no injury, except a slight contusion in the hand. But Dr. VANDIER, a French gentleman of considerable chemical and medical talents, to whom I am indebted for much able assistance in my laboratory, was wounded in several places; particularly, the tunica conjunctiva of the eye was so lacerated, that a piece of it hung down, and, by getting under the inferior eyelid, caused the most painful irritation, and endangered his sight. One of the frontal arteries also was divided. I relate these circumstances thus fully, as the most effectual means of putting upon their guard, those who would repeat the experiment. If the sulphuric acid be dilute, heat may be applied with more safety; and the phenomena are different. The hyperoxygenized muriatic acid is disengaged from the basis; but, as the heat requisite to distil the acid is more than sufficient to decompose it, oxygenized muriatic acid comes over with it; and oxygen gas is collected in the pneumatic tub. If the distillation be continued, the same danger arises as in the former case, because the sulphuric acid becomes concentrated; and it would seem, that its action upon the salt is slight and partial at a low temperature, but violent and instantaneous when heated and concentrate. I could not, therefore hope, by these means, to obtain the acid disengaged and pure.

If the manner of bringing the sulphuric acid and the salt into contact be reversed, and the salt be dropped into the acid, the yellow vapours and the orange-coloured liquor are produced, but generally without decrepitation. If they be allowed to remain some days in contact, the vapours continue, and oxygen gas is constantly disengaged, even in the common light of the

day, and at the temperature of the atmosphere. By cooling the first receiver with ice, I thought that I had once obtained this acid crystallized in the form of four-sided pyramids, of an orange colour. But, though I really believe this to have been the case, I do not positively affirm it.

Nitric acid produces nearly the same phenomena; but the smell and other properties are rather less distinct and marked, than with sulphuric acid.

Muriatic acid decomposes this salt, and unites to its basis; but neither the yellow vapours, nor the orange-coloured liquor, are produced. The circumstances which attend the contact of the acid and the salt, are as follows. If no more muriatic acid be present than is merely necessary to decompose the salt, I do not doubt that hyperoxygenized muriatic acid will be driven off, as little decomposed as with the other acids, supposing the action to be instantaneous; but, during the contact of these two bodies, the acid expelled must meet muriatic acid not yet combined, and, uniting with it, always forms a portion of oxygenized muriatic acid. The quantity of the last acid must vary, according to the quantity of muriatic acid employed, and not combined with the alkali. It was by this method that Mr. CRUICKSHANK obtained the muriatic acid gas, which he stated to contain 43,5 per cent. of oxygen.

Phosphoric and arsenic acids do not act upon this salt, till heated with it; and then much oxygen gas is evolved. These, therefore, afford no better method of disengaging hyperoxygenized muriatic acid without decomposition.

Oxalic, tartareous, and citric acids, act as I before mentioned; and the hyperoxygenized muriatic acid holds its place, in the order of affinities for potash, immediately before the benzoic.

I shall not stop to detail a number of amusing phenomena that may be produced, by projecting into the stronger acids, mixtures of combustible bodies, whether metallic or not, and hyperoxygenized muriate of potash. The cause of them is well understood, and the theory points them out: they are, therefore, no longer objects of philosophical admiration. But I must mention one experiment, which, had it succeeded, I should have thought important. I projected various mixtures of very minutely pulverised diamond and this salt, into the different acids; but found the diamond undiminished, by every attempt to combine it with oxygen in the humid way.*

Another, but imponderable, part of this salt, as indeed of all hyperoxygenized muriates, seems to be an extraordinary quantity of caloric. For, during their formation, scarcely any heat is disengaged, as by other acids; and, very little heat applied to the salts, gives the gaseous form to their oxygen.

An opinion has prevailed among some ingenious chemists, that, from a mixture of this salt with sulphuric acid, nitrous gas is disengaged, and sulphate of lime formed in the retort. But this is a mistake, arising, on the one hand, from the smell and vapour of the hyperoxygenized muriatic acid, and, on the other, from sulphate of lead, which the common sulphuric acid of this

* I must confess, that the vivid flashes of light, emitted from the mixture of this salt and combustible bodies thrown into an acid, appear to me, in some measure, to prove the modification proposed by LEONHARDI, RICHTER, GREN, &c. to that part of the LAVOISIERIAN theory which regards the emission of light during combustion. Another testimony in favour of their modification, may be drawn from the vegetable kingdom. All plants growing in places deprived of light, are merely mucilaginous. But the mucilage of these plants burns without the emission of light. Light, therefore, appears not to be disengaged from oxygen; else, why not by this mucilage, as well as by other combustible bodies?

country frequently contains in solution, and which is precipitated from it by water. Before we assert a fact, we should be well assured of the pureness of our chemical agents. This supposed conversion of muriatic or hyperoxygenized muriatic acid into nitrous gas, will not pass for a decomposition, or a transmutation, of that refractory radical; and the idea of the change of potash into lime, is as erroneous as some other late assertions respecting the decomposition of the alkalis.

The proportions of this salt are, as I before stated,

Hyperoxygenized muriatic acid - -	58,3
Potash - - - - - -	39,2
Water - - - -	2,5
	100,0.

2d Species. Hyperoxygenized Muriate of Soda.

This salt is prepared in the same manner, and with the same phenomena, as the former. It is extremely difficult to obtain it pure, as it has nearly the same degree of solubility in water as muriate of soda. It is soluble in three parts of cold, and less of warm water; and is slightly deliquescent. It is soluble in alcohol; but this property alone is not sufficient to enable us to obtain it free from the muriate of soda, formed along with it in the entire liquor; as the latter salt, contrary to the assertions of all authors, is soluble in alcohol, and seems to be much more so, when accompanied by the hyperoxygenized muriate. It was by taking a large quantity of the entire salt, formed by sending a current of oxygenized muriatic acid gas through a solution of carbonate of soda, and repeatedly crystallizing in alcohol, that, with great difficulty, I obtained a little pure hyperoxygenized muriate of soda. It crystallizes in cubes, or in rhomboids little

different from cubes. It produces a sensation of cold in the mouth; and its taste is easily distinguished from muriate of soda. It is decomposed by heat, by combustible bodies, and by acids, in the same manner as the former species; and the acid holds its place for soda, as for potash, immediately before the benzoic. The basis is separated by potash only. This salt is composed of,

Hyperoxygenized muriatic acid - -	66,2
Soda - - - -	29,6
Water - - - -	4,2
	100,0.

3d Species. Hyperoxygenized Muriate of Barytes.

The earthy bases seem to follow, in the order of affinities for this acid, at a great distance from the alkalis. They are all superseded by the two just mentioned; and it is much more difficult to accomplish their union with the acid, than is the case with potash or soda. The most advantageous method is, to pour warm water upon a large quantity of this earth, procured by Mr. VAUQUELIN's method; and to cause a current of oxygenized muriatic acid to pass through the liquor, kept warm; so that the barytes already dissolved being saturated, a fresh portion of it may be taken up by the water, and presented in a state of great division to the acid. This salt is soluble in about four parts of cold, and less of warm water. It crystallizes like the muriate of this earth; and resembles it so much in solubility, that I could not separate them effectually by crystallization repeated several times. At first, indeed, I despaired of ever obtaining any of the earthy hyperoxygenized muriates in a state sufficiently pure for analysis. If we consider them as a genus distinct from the alkaline hyperoxygenized muriates,

a leading character may be, their great resemblance to their respective species of earthy muriates. I thought, however, that I might, if not by direct, at least by double affinity, decompose the one without the other; and phosphate of silver occurred to me as the most likely agent. If phosphate of silver be boiled with muriate of lime, of barytes, &c. a double decomposition ensues; and muriate of silver, together with phosphate of the earth, both insoluble, are precipitated. To increase the action, the phosphate of silver may be dissolved in a weak acid, such as the acetous; and, though the earthy phosphate be at first retained in solution, it will be separated by expelling the acid. The only condition absolutely necessary is, that the silver employed be free from copper. For, in preparing phosphate of silver by phosphate of soda, and by nitrate of silver thus impure, copper would be thrown down by the phosphoric acid; and the phosphate of copper would be afterwards decomposed by muriate of lime. Muriate of copper would therefore remain with the earthy hyperoxygenized muriates; or, what is still worse, a part of the muriatic acid being easily expelled from oxide of copper, the hyperoxygenized muriatic acid would be driven off from its basis, by the more powerful agency of the former. This salt has all the properties enumerated as belonging to the genus of hyperoxygenized muriates; and, with heat, the acid is expelled by all acids above the benzoic. I had hoped that, without distillation, I could procure the acid from the salt by means of sulphuric acid, which would have left an insoluble salt with barytes; but hyperoxygenized muriatic acid is so easily decomposed by light, that I have not yet obtained it, to my satisfaction, disengaged and pure. A fact well worthy of attention is, that the stronger acids disengage this acid with a flash of light,

more frequently from the earthy than from the alkaline hyperoxygenized muriates; a phenomenon which, I suppose, depends upon the relative proportionate affinities, and consequently the greater rapidity of the disengagement. But, where all is hypothesis, it is useless to draw any inference from a single fact.

The proportions of this salt are,

Hyperoxygenized muriatic acid - -	47
Barytes - - - -	42,2
Water - - - -	10,8
	100,0.

4th Species. Hyperoxygenized Muriate of Strontia.

The foregoing observations apply to the formation of this salt, to the mode of obtaining it pure by phosphate of silver, to its conduct with the acids, to the rank of its acid in the order of affinities, and to its other properties. It is deliquescent; and more soluble in alcohol than muriate of strontia. It melts in the mouth immediately, and produces cold. Its crystals assume the shape of needles.

It is composed of,

Hyperoxygenized muriatic acid - -	46
Strontia - - - -	26
Water - - - -	28
	100.

5th Species. Hyperoxygenized Muriate of Lime.

This salt is obtained pure, in the same manner as the other earthy salts. It is extremely deliquescent; liquifies at a low heat, by means of its water of crystallization; and is very

soluble in alcohol. It produces much cold, and a sharp bitter taste in the mouth.

It is composed of,

Hyperoxygenized muriatic acid - -	55,2
Lime - - - -	28,3
Water - - - - -	16,5
	100,0.

6th Species. Hyperoxygenized Muriate of Ammonia.

From the property which oxygenized muriatic acid possesses of decomposing ammonia, this combination may be thought paradoxical. For, how can an acid much more active than oxygenized muriatic acid exist with ammonia, which is destroyed by the latter? But this argument may be opposed by the sum of affinities that act in either case. If the affinity of composition of oxygenized muriatic acid and of ammonia, together with the affinity of oxygenized muriatic acid for ammonia, to form oxygenized muriate of ammonia, be not more powerful than the affinity of oxygen for hydrogen, of azote for caloric, and of muriatic acid for ammonia, the divellent affinities will prevail; and this is what actually happens. But, although oxygen may be held with less force of attraction in oxygenized than in hyperoxygenized muriatic acid, yet the affinity of the latter acid for ammonia may increase in a much greater ratio, and favour the quiescent affinities. If carbonate of ammonia be poured into any earthy salt of this genus, a double decomposition takes place; and hyperoxygenized muriate of ammonia is formed. This salt is very soluble in water, and in alcohol. It is decomposed at a very low temperature, and gives out a quantity

of gas, together with a smell of hyperoxygenized muriatic acid. Such a smell is doubtless owing to the great quantity of oxygen contained in the acid; it being more than is necessary to combine with the quantity of hydrogen contained in the alkali, and therefore some of the acid is disengaged, without decomposition. All the attempts I have made to ascertain the proportions of its principles, have been fruitless. The formation and existence of this salt, as I before said, are very strong proofs of what I have advanced respecting the state in which hyperoxygenized muriates at first exist; and very fully prove the different degree of affinity exercised by each acid toward the basis.

7th Species. Hyperoxygenized Muriate of Magnesia.

Its chemical and physical properties are nearly the same with those of the 5th species, only that, in addition to the other bases, lime and ammonia cause a precipitate in this salt.

Its proportions are,

Hyperoxygenized muriatic acid - -	60
Magnesia - - - -	25,7
Water - - - -	14,3
	100,0

8th Species. Hyperoxygenized Muriate of Alumina.

I put some alumina, precipitated from muriate of alumina, and well washed, but still moist, into a WOULFE's apparatus, disposed as for the other earths, and sent a current of oxygenized muriatic acid gas through the liquor. The alumina shortly disappeared; and, upon pouring sulphuric acid into the liquor, a strong smell of hyperoxygenized muriatic acid was perceivable. When I attempted to obtain the salt pure, by phosphate of silver, in the

usual way, I found nothing in solution but hyperoxygenized muriate of silver;* and all the hyperoxygenized muriate of alumina had been decomposed. This salt, however, appears to be very deliquescent, and is soluble in alcohol; but I could not ascertain the proportion of its principles, because I did not obtain it sufficiently free from the simple muriate.

9th Species. Hyperoxygenized Muriate of Silica.

I am inclined to think this salt does not really exist. A current of oxygenized muriatic acid, sent through some silica which had been precipitated from an acid by ammonia, and collected moist from the filter, did not seem to dissolve any portion of it. In all barytes and strontia, prepared according to Mr. VAUQUELIN's method, a portion of silica from the crucibles is attacked, and taken up, by whatever acid those earths may afterwards be dissolved in; and, in all potash of commerce, there is some silica; but I have never perceived that any portion of this earth had been dissolved by this acid.

The very small portion of earth which, in attempts to form the different species of this genus of salts, is taken up by acids, and the still smaller portion of the salt so formed, which is really in the state of hyperoxygenized muriate, render the operation so tedious, that I have confined myself to form what was necessary to determine their analysis, in such a manner as I believe to be nearly accurate. It cannot, therefore, be expected that I make myself responsible, without a right of appeal to further experiments, for the accuracy with which the crystalline forms, and other physical properties,

* This salt shall be particularly mentioned and described in another part of this Paper. For the present, it is sufficient to say, that it is very soluble in water; and, in that property, as in many others, is totally different from muriate of silver.

may have been stated. It is impossible to obtain satisfactory crystals from a very small portion of salt; and I have attached myself more particularly to chemical than to physical characters, as being a much more important and certain mode of determination. For the same reason, I have not examined the combination of the new and rarer earths with this acid. But I do not doubt, that whatever chemist undertakes a further investigation of these extraordinary bodies, will be amply repaid for his labour.

I have mentioned, in a former part of this Paper, that all muriates lost a portion of their acid at a red heat. I exposed one hundred parts of muriate of potash, in a crucible, to a red heat, for some minutes, and found that they lost five. I dissolved them in water, and they manifested alkaline properties. Treated by nitrate of silver, they gave a precipitate, which shewed one per cent. less of muriatic acid, than 100 parts of the same salt that had not been exposed to fire. A violent heat may be necessary to expel the last portion of water of crystallization from certain salts, as we know particularly is the case with sulphate of lime. But, if any of the acid can be expelled at the same temperature, there is no longer any certainty. The quantity of water, as stated by different chemists, varies much; and, from some experiments I have made, I do not believe it to have been accurately determined. The method I used to ascertain this, was as follows: I exposed a given quantity of the salt to a violent heat, and noted its loss of weight. I then precipitated, by nitrate of silver; and thus knew, how much the quantity of muriatic acid which this salt contained, was less than that in a like portion which had not been exposed to heat. I subtracted the difference in this quantity, from the total loss of weight in the

salt exposed to heat; and the remainder I considered as water. It was upon results obtained in this manner, that I founded many of the proportions I have given in this Paper.

It is stated in the tables of BERGMAN, corrected by Dr. PEARSON, that lime and strontia prefer acetous to arsenic acid. But arsenic acid can expel hyperoxygenized muriatic acid from its basis, although the acetous cannot act in the same manner; therefore, this order of affinities is erroneous. It was not till lately, that we had potash and soda so pure as to be relied upon in delicate experiments; and it is not surprising that we find mistakes with regard to their taking the acid from barytes, strontia, and lime. But real potash and soda both precipitate even barytes from hyperoxygenized muriatic acid. If ever it becomes easy to obtain hyperoxygenized muriate of barytes, we may prepare that earth from it in the humid way, and more near to purity, than in the method proposed by VAUQUELIN.

METALLIC COMBINATIONS OF MURIATIC ACID, IN ITS DIFFERENT STATES.

The action of hyperoxygenized muriatic acid upon metals, is, as may well be expected, rapid, and without disengagement of gas. It appears to dissolve every metal, not excepting gold and platina. If the metal be presented to the acid at the moment when it is disengaged from the salt, inflammation ensues; and the phenomena of light and heat vary according to the metal; but the salts thus produced are merely muriates. In order to form real hyperoxygenized muriates, it is necessary to take the metal in its fullest state of oxidizement, and combine it with the acid, either by double decomposition, or by passing a current of oxygenized muriatic acid gas through the oxide suspended

in water. The acid is thus separated into muriatic and hyperoxygenized muriatic acid; and, in these states, combines with the metallic oxide. The metallic hyperoxygenized muriates are different, in every respect, from the metallic muriates. Red oxide of iron is dissolved with difficulty. Oxide of copper more easily. Red oxide of lead exhibits the same appearances, during its combination with this acid, as with nitric acid. When nitric acid is poured, even in excess, upon red oxide of lead, only a part of the oxide is dissolved, unless heat be applied; and what remains becomes a blackish brown powder. But, if metallic lead be added, in a just proportion, all the red oxide disappears, and none of the brown powder is formed; neither is there any disengagement of nitrous gas, when the metallic lead is dissolved. The precipitates caused in either case, by pouring an alkali into the nitric solution, are yellow. Hence it appears, that red oxide of lead contains too much oxygen to be dissolved by nitric acid. One part of the oxide takes up the excess of oxygen, and becomes brown; while the portion which loses oxygen, becomes yellow, and is soluble in nitric acid. The presence of metallic lead promotes the total solution of the red oxide, by taking up the superabundant oxygen. I found that a current of oxygenized muriatic acid gas, like the nitric acid, dissolved a part of the red oxide, and caused the brown powder to be formed, upon which it could not act. Hyperoxygenized muriate of lead is much more soluble than muriate of lead; and the acid is very slightly attracted by the basis.

But, of all the metallic salts formed by the combination of the muriatic acid, in any of its different states, none so much deserve attention as those which have for their bases, the oxides of mercury. The nature of the salts which result from the

combination of common muriatic acid with the different oxides of this metal, has been stated in the most contradictory manner, by different chemists. But, as the knowledge of hyperoxygenized muriatic acid has thrown some light upon the true state of calomel and corrosive sublimate,* I must beg leave to dwell at some length upon this important part of my subject.

It would be useless to repeat the opinions of the old authors, who have treated of corrosive sublimate, and of calomel. They are to be found in the works of those respective chemists, and I must refer to them for particulars.

In the Memoirs of the Academy of Sciences of Paris, for 1780, we find a Paper of Mr. BERTHOLLET, upon the causticity of metallic salts; in which he appears to think, that the acid in corrosive sublimate is in the state of what was then called dephlogisticated marine acid. In 1785, when he had examined the oxygenized muriatic acid with more care, he renounced his former opinion; and gave the reasons why he no longer adhered to it. Some late experiments of Mr. PROUST shew, that this chemist thinks as Mr. BERTHOLLET now does. And these may be ranked among the first of modern authorities.

Notwithstanding those opinions, Mr. FOURCROY, in his *Système des Connoissances chimiques*, still considers corrosive sublimate as a hyperoxygenized muriate of mercury; and designs it

* I regret very much, that I am under the necessity of using these unmeaning terms. But the French nomenclature has made no distinction between salts formed by metallic oxides in different states of oxidizement, except by the colour, which is an extremely defective and unmeaning method. At all events, this metal is so uncomplaisant as to retain the white colour, in its different oxides combined with muriatic acid. I prefer, however, using the old name, to proposing any provisional substitute that might be found defective. This will be farther explained in *Remarks upon chemical Nomenclature.*

throughout by that name.* This chemist, one of the founders of the methodical Nomenclature, is too well acquainted with its principles, to apply the term hyperoxygenized muriate to any thing but a combination of hyperoxygenized muriatic acid. It is evident, therefore, that he considers the portion of oxygen, which, in equal quantities of corrosive sublimate and calomel, is greater in the former, to be combined with the acid, and not with the oxide of mercury. As soon as I have stated some experiments that prove Mr. FOURCROY's opinion to be erroneous, and endeavoured to establish the analysis of corrosive sublimate and of calomel, I shall take notice of a salt hitherto unknown, which really is hyperoxygenized muriate of mercury.

I took a portion of corrosive sublimate, and precipitated by potash. The liquor was filtered; and, upon being tried, nothing but muriate of potash was found. No reagent could discover the smallest trace of hyperoxygenized muriatic acid.

Sulphuric, nitric, phosphoric, and many other acids, poured upon corrosive sublimate, did not disengage either muriatic, or hyperoxygenized muriatic acid. Nitrate of silver, poured into a solution of corrosive sublimate, gave an abundant white precipitate.

From these experiments it is evident, that muriatic acid, not hyperoxygenized muriatic acid, is combined with the oxide of mercury in corrosive sublimate.

To determine the proportions of this salt, I took one hundred parts, and precipitated by nitrate of silver. I then took another hundred, and precipitated by potash. The result of these two

* I have said before, that this acid was talked of by many chemists, as if the existence of it had really been proved.

experiments was such as to establish the proportions of corrosive sublimate as follows:

Oxide of mercury - - -	82
Muriatic acid - - - -	18
	100.

But, the acid of this salt not being charged with a superabundance of oxygen, we must look for the excess in the metallic oxide. I took 100 grains of mercury, and dissolved them in nitric acid; then poured in muriatic acid; and, at a very gentle heat, evaporated to dryness. I afterwards sublimed, in a Florence flask, the salt that remained, and obtained 143,5 of corrosive sublimate. But, 143,5 of corrosive sublimate, contain 26 of acid; which will leave 117,5 for the mercurial oxide; and, if 117,5 contain 100 of mercury, 100 of the oxide will contain 85. Therefore, the oxide of mercury, in corrosive sublimate, is oxidized at the rate of 15 per cent.

To determine the proportions in calomel, I dissolved 100 grains of it in nitric acid. The phenomena of the solution have been so accurately described by Mr. BERTHOLLET, that I shall not repeat them. I precipitated by nitrate of silver; and obtained a quantity of muriate of silver, corresponding with 11,5 of muriatic acid. The oxide of mercury I obtained apart. Therefore, calomel is composed of,

Oxide of mercury - - -	88,5
Muriatic acid - - - -	11,5
	100,0.

To ascertain the state of oxidizement of the oxide in calomel, I took 100 grains, and boiled them with nitro-muriatic acid;

then evaporated very slowly, and sublimed as above. The calomel was totally converted into corrosive sublimate, and weighed 113. But 113 of corrosive sublimate contain 20,3 of muriatic acid, of which, 11,5 were originally in the calomel. The total addition of weight was 18. But the quantity of acid in these 18, amounts to 20,3 — 11,5 = 8,8. Therefore, 18 — 8,8 = 4,2, remain for that part of the additional weight which is oxygen. On the other hand, 100 of calomel contain the same quantity of mercury as 113 of corrosive sublimate, = 79. These 79, with 11,5 of acid, are equal to 90,5, and leave 9,5 for the quantity of oxygen contained in calomel. It would appear, from these experiments, that corrosive sublimate contains 6,5 per cent. more acid, and but 2,8 per cent. more oxygen, than calomel. But this quantity of oxygen is combined with a much greater proportion of mercury; and forms an oxide of a very different degree of oxidizement. For, 88,5 : 9,5 :: 100 : 10,7. Therefore, we may establish the following comparative table.

CALOMEL.		CORROSIVE SUBLIMATE.	
The oxide of mercury in calomel is composed of,		The oxide of mercury in corrosive sublimate is composed of,	
Mercury - -	89,3	Mercury - - -	85
Oxygen - -	10,7	Oxygen - -	15
	100,0.		100.
And calomel is composed of,		And corrosive sublimate is composed of,	
Mercury 79 Oxygen 9,5 (oxide of mercury)	88,5	Mercury 69,7 Oxygen 12,3 (oxide of mercury)	82
Muriatic acid -	11,5	Muriatic acid -	18
	100,0.		100.

These proportions are different from those given by LEMERY, GEOFFROY, BERGMAN, &c. But, without calling in question the accuracy and skill of these chemists, it is fair to assert, that the pure materials used by modern chemists, are more likely to lead to sure results, than the impure reagents of the ancients.

In these salts we find another instance, that, in proportion as metallic oxides contain a greater quantity of oxygen, they require a greater quantity of acid to enter into combination with them.

The method I have followed, to ascertain the proportions just stated, may appear, at first view, not to be the shortest that I might have adopted. But I have tried others, and have found none so accurate. It is impossible, synthetically, to convert a given quantity of mercury into calomel, in such a manner as to be certain that none of it is in a different state from that required. And, if we would attack calomel analytically, the action of the alkalis, without which we cannot proceed, is such as to alter the nature of the oxides. I have also made many comparative experiments, by dissolving calomel in nitro-muriatic acid, (which converted it into corrosive sublimate,) and then precipitating by ammonia; but I have not found these trials so successful as those I have described. The nature of the precipitate from corrosive sublimate by ammonia, certainly differs, according to the excess of acid that may be present; and mercury seems to have the power of existing in many degrees of combination with oxygen. The only precaution absolutely necessary, in this mode of operating, is, that while the mercurial salt is in an open vessel, it should not be exposed to a degree of heat capable of volatilizing any part of it.

The quantity of mercury ordered in the London Pharmacopoeia, to convert corrosive sublimate into calomel, is 9 pounds

of mercury for every 12 pounds of corrosive sublimate. But, from the above experiments, it would appear, that a smaller quantity of mercury might strictly answer. However, from the results of minute investigation, we should not conclude too hastily upon preparations on the great scale; and, I rather think, that the excess of mercury ordered by the Pharmacopoeia is a useful precaution.

In my experiments, I attempted to reduce, by means of copper, iron, or zinc, the mercury contained in the mercurial salts. Iron did not answer the purpose: zinc precipitated the mercury a little better; and copper produced a change which I did not expect. If a bit of copper be put into a solution of corrosive sublimate, a white powder shortly falls to the bottom; and that powder is calomel. When washed, it does not contain an atom of copper, nor of corrosive sublimate.

Before I conclude these considerations, I must say, that whether calomel be prepared in the dry or in the humid way,* it does not seem to differ chemically; nor does it contain any

* By the humid way, I do not mean precisely the method of SCHEELE. That chemist desires us to boil the acid with the mercury, after they have ceased to act upon each other at a low temperature. By this method, the nitric acid takes up an excess of mercurial oxide; and the nitrate of mercury thus formed, precipitates by water. Therefore, when this nitrate of mercury is poured into the dilute solution of muriate of soda, according to the formula of SCHEELE, the action, on the part of the solution, is twofold.

1st. The water acts upon one part, and precipitates an oxide, or rather an insoluble subnitrate of mercury. And,

2dly. A double decomposition takes place between the nitrate of mercury and the muriate of soda. It is with reason, that the medical world have supposed the calomel of SCHEELE to be different from that prepared in the humid way; for it is, in fact, calomel, plus an insoluble subnitrate of mercury. In the first part of SCHEELE's

sensible portion of water of crystallization. The same may be said of corrosive sublimate.

It now remains to speak of the real hyperoxygenized muriate of mercury. I passed a current of oxygenized muriatic acid gas through some water, in which there was red oxide of mercury.* After a short time, the oxide became of a very dark brown colour; and a solution appeared to have taken place. The current was continued for some time; and, when I thought that a sufficient quantity of the oxide had been dissolved, I stopped the operation. The liquor was evaporated to dryness; and the salt was thus obtained. There evidently was in the mass a great proportion of corrosive sublimate, as might be expected, from what I had observed to take place in the formation of the other salts of this acid; but, by carefully separating

process, there is disengagement of nitrous gas, together with oxidizement and solution of some of the mercury. When he boils the acid upon the remaining mercury, there is no further disengagement of gas; yet more mercury is dissolved. The nitrate of mercury, therefore, rather contains an oxide less oxidized after ebullition than before it. The true difference is in the subnitrate of mercury, precipitated, as I before said, by the water in which the muriate of soda was dissolved. And the orange coloured powder, which remains after an attempt to sublime SCHEELE's calomel, is to be attributed to the same cause. To prepare calomel in the humid way, uniform as to itself, and in all respects similar to that prepared in the dry way, it is necessary, either to use the nitric solution before it has boiled, or to pour some muriatic acid into the solution of muriate of soda, previously to mixing it with the boiled solution of nitrate of mercury. In the first case, no precaution is necessary; and, in the latter, the oxide of mercury, which the nitrate of mercury has, by boiling, taken up in excess, finds an acid which is ready to saturate it. All the mercurial oxide being thus converted into calomel, none of that subnitrate of mercury can be present.

The objections made by a medical gentleman against SCHEELE's calomel, when this Paper was read before the Royal Society, led me to reconsider the subject, and to undertake the investigation detailed in this note.

* I used either of the red oxides of mercury, indiscriminately.

the last formed crystals, I could pick out some hyperoxygenized muriate of mercury. I then crystallized it over again; and, in this manner, I obtained it nearly pure. This salt is more soluble than corrosive sublimate: about four parts of water retain it in solution. The shape of its crystals, I cannot well determine. When sulphuric, or even weaker acids, are poured upon it, it gives out the usual smell of hyperoxygenized muriatic acid; and the liquor becomes of an orange colour. This is a sufficient proof, that corrosive sublimate is not a hyperoxygenized muriate of mercury.

I have just mentioned that, in the formation of this salt, the oxide of mercury, which was not dissolved by the acid, became of a very dark brown colour. I procured a portion of this oxide, which seemed different from the red oxide. It however retained the form, and the crystalline appearance, of the latter. It was soluble in nitric acid, without disengagement of gas; and was precipitated from it, in a yellow oxide, by all the alkalis, except ammonia. It formed corrosive sublimate with muriatic acid; and the precipitate by the alkalis, was the same as that from corrosive sublimate, made with the red oxide. Yet I am inclined to think, that the dark brown oxide differs in some essential point from the red; but I have not yet made sufficient experiments to prove this opinion. At all events, the present object being to examine the mercurial oxides only as combined with muriatic acid, it would be foreign to the purpose, to enter upon too minute an investigation of the other states of the metal. This, and some other objects hinted at in this Paper, must be reserved for future inquiry.

In treating the earthy hyperoxygenized muriates with phosphate of silver, as I mentioned before, I observed that the liquor

sometimes contained in solution oxide of silver; which, upon examination, I found to be combined with hyperoxygenized muriatic acid. As the salt which is thus formed is different, in every respect, from simple muriate of silver, it may be of some importance to consider it with attention. In the first place, it will afford the most convincing proof of the difference between muriatic and hyperoxygenized muriatic acid; and, in the next place, it particularly deserves to be remarked, for possessing, in the most eminent degree, one of the great characteristic features of the genus to which it belongs. Hyperoxygenized muriate of silver is soluble in about two parts of warm water; but, by cooling, it crystallizes in the shape of small rhomboids, opaque and dull, like nitrate of lead or of barytes. It is somewhat soluble in alcohol. Muriatic acid decomposes it; as does nitric, and even acetous acid: but the result of this decomposition is not, as might be expected, nitrate or acetite of silver. At the moment that the acid is expelled from hyperoxygenized muriate of silver, a reaction takes place among its elements: oxygen is disengaged; and the muriatic acid remains in combination with the oxide of silver. If this fact be compared with the manner in which nitric and acetous acids act upon hyperoxygenized muriate of potash, it will give a strong proof of the proportionate affinities of all these acids for oxide of silver, in comparison with that which they exercise towards the alkali.

Hyperoxygenized muriate of silver, when exposed to a very moderate heat, begins by melting, and then gives out a considerable quantity of oxygen gas, with effervescence; and muriate of silver remains behind. These phenomena however differ much, according to the degree of heat applied. When hyperoxygenized muriate of silver is mixed with about half its weight

of sulphur, it detonates in the most violent manner; and does not, like hyperoxygenized muriate of potash, require the addition of charcoal, to possess a very great force of explosion. The slightest pressure is sufficient to cause this mixture to detonate; and I think I shall be within bounds, when I state, that half a grain of hyperoxygenized muriate of silver, with a quarter of a grain of sulphur, explodes with a violence at least equal to five grains of hyperoxygenized muriate of potash, with the due quantities of sulphur and charcoal. The flash is white and vivid, and is accompanied by a sharp and quick noise, like the fulminating silver so ably desctibed by Mr. HOWARD; and the silver is reduced to the metallic state, and vaporized.

I think it right to add a few remarks, upon what I have termed the proportionate affinities of acids and of bases, one for the other. It is a law, not indeed universally, but frequently observed, and very well worthy of consideration, that the acids are attracted by metallic oxides, in a very different order from that in which they are disposed to unite to alkaline and earthy bases.

Nitric acid, which holds so high a place in the order of affinities for alkalis, is expelled from metallic oxides by most acids. Phosphoric, fluoric, all the vegetable acids, except two or three, and the animal acids, attract the latter bases more strongly. Nay, we shall find, upon an attentive examination, that acids commonly attract metallic oxides, in the inverse ratio of their action upon metals, or, in other words, in proportion to their own affinity of composition. Thus, the phosphoric and fluoric acids sometimes rank before the sulphuric; and the nitric, as I before said, is generally very low. Hyperoxygenized muriatic acid seems to follow the same rule; and takes its

place, in the order of affinities for metallic oxides, after many of those acids which it can expel from earths and alkalis.

The other hyperoxygenized muriates, I have not yet sufficiently examined. I shall, however, mention at present, that I have ascertained the muriatic salts, formerly known by the strange name of *butters of the metals*, to be muriates, and not hyperoxygenized muriates; and the extraordinary proportion of oxygen, to be combined, not in the acid, but in the metallic oxide.

In the course of different experiments, I have known hyperoxygenized muriatic acid to be formed in two cases, where I could not have expected it.

In the analysis of some menachanite from Botany Bay, given to me last year by the President of the Royal Society, I observed, that while the oxide of titanium was precipitated from the muriatic acid in which it was dissolved, the excess of oxygen in the oxide passed over to the muriatic acid and the potash, already in the liquor, and that hyperoxygenized muriate of potash was formed. I have attempted the same experiment with black oxide of manganese, but could not succeed.

There is, however, a still more extraordinary formation of this acid, in the distillation of nitro-muriatic acid upon platina. Oxygen is absorbed by the metal; yet, not only oxygenized, but also hyperoxygenized muriatic acid is formed. I have repeated the experiment several times; and am well convinced of the fact, however contrary to theory it may appear. I have tried the action of oxygenized muriatic acid upon nitric acid, in the hopes of forming hyperoxygenized muriatic acid; but there was no action to this effect among their elements.

The fact of the production of a peculiar gas, by the distilla-

tion of nitro-muriatic acid upon platina, has been observed by Mr. DAVY, in his *Researches*.* But, as hyperoxygenized muriatic acid was not known at that time, he could not say the real nature of that gas. Had Mr. DAVY carried his ingenious experiments a little farther, we should have been much earlier acquainted with the last degree of oxygenizement of muriatic acid.

Mr. BERTHOLLET terminates his Paper upon hyperoxygenized muriate of potash, by saying, that he will consider muriatic acid as the radical; oxygenized muriatic acid, as corresponding with sulphureous and nitrous acid; and the acid which he conjectured to exist in this salt, as corresponding with sulphuric and nitric acid. I shall now conclude, by stating the arguments in favour of each denomination, and the analogies upon which they are founded.

Muriatic acid is for us a simple body; but it has acid properties of the strongest kind; therefore, from analogy, we suppose it to contain oxygen. But may not this be too hasty a conclusion? Are we not very doubtful concerning the existence of oxygen in prussic acid? And are we not, on the contrary, certain that sulphurated hydrogen, which possesses many of the characteristics of acids, does not contain any? Of the oxygenizement of fluoric and boracic acids, we have no proof: but then we cannot affirm that any one of these acids exists in three states of combination with oxygen; and the muriatic is the only radical of which we admit this fact. We must not, however, pretend to limit the number or degrees of combinations between combustible bodies and oxygen; but we can

* Dr. PRIESTLEY, also, mentions a peculiar gas, produced by distilling a solution of gold in *aqua regia*.

speak, with certainty, only of those things which are proved. Besides its acid properties, this substance has others, common to oxygenizable bodies. With 16 of oxygen, it forms an acid, which, in many of its properties, is to its radical what the sulphureous is to sulphur. Like the sulphureous, it is volatile; has little attraction for salifiable bases; destroys vegetable blues; and is capable of further oxygenizement. With 65 of oxygen, it becomes more fixed, like sulphuric acid; has a stronger affinity for salifiable bases; and acquires more truly acid properties. Upon these considerations, I submit to the chemical world, whether, in the present state of our knowledge, it be not more philosophical to say,

Muriatic radical, or some single word of the same import, Muriatous acid, Muriatic acid,	instead of	Muriatic acid; Oxygenized muriatic acid; Hyperoxygenised muriatic acid.

I am fully aware that, at first sight, this may appear extraordinary; and the more so, as we have no positive facts that prove muriatic acid to be a simple body. All we can, therefore, consider fairly, is, in favour of which appellation does the sum of analogies seem to preponderate. And, to give the cause a candid investigation, we should begi by considering, whether the presence of oxygen in all bodies that have acid properties, has been rigidly demonstrated; and not determine by this law of the French chemistry, till we are well convinced it has not been too generally assumed.

If a nomenclature be not subservient to the uses of science, and does not keep pace with its progress, the relation between

substances and their names will become so relaxed, that confusion will be brought about, by the very means we take to avoid it; and if, while we continue to extend our acquaintance with chemical bodies, nomenclature remains confined within its former limits, the bonds that unite these two parts of the science must inevitably be broken.

VII. *Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron.* By Edward Howard, *Esq. F. R. S.*

Read February 25, 1802.

THE concordance of a variety of facts seems to render it most indisputable, that certain stony and metalline substances have, at different periods, fallen on the earth. Whence their origin, or whence they came, is yet, in my judgment, involved in complete obscurity.

The accounts of these peculiar substances, in the early annals, even of the Royal Society, have unfortunately been blended with relations which we now consider as fabulous; and the more ancient histories of stones fallen from heaven, from Jupiter, or from the clouds, have evidently confounded such substances with what have been termed *Ceraunia, Bœtilia, Ombria, Brontia,* &c. names altogether unappropriate to substances fallen on our globe. Indeed some mislead, and others are inexpressive.

The term Ceraunia, by a misnomer, deduced from its supposed origin, seems, as well as Bœtilia,* to have been anciently used to denote many species of stones, which were polished and shaped into various forms, though mostly wedge-like or triangular, sometimes as instruments, sometimes as oracles, and sometimes as deities. The import of the names, Ombria, Brontia, &c. seems subject to the same uncertainty.

In very early ages, it was believed, that stones did in reality

* MERCATI, Metallotheca Vaticana. page 241.

fall, as it was said, from heaven, or from the gods; these, either from ignorance, or perhaps from superstitious views, were confounded with other stones, which, by their compact aggregation, were better calculated to be shaped into different instruments, and to which it was convenient to attach a species of mysterious veneration. In modern days, because explosion and report have generally accompanied the descent of such substances, the name of thunderbolt, or thunderstone, has ignorantly attached itself to them; and, because a variety of substances accidentally present, near buildings and trees struck with lightning, have, with the same ignorance, been collected as thunderbolts, the thunderbolt and the fallen metalline substance have been ranked in the same class of absurdity. Certainly, since the phenomena of lightning and electricity have been so well identified, the idea of a thunderbolt is ridiculous. But the existence of peculiar substances fallen on the earth, I cannot hesitate to assert; and, on the concordance of remote and authenticated facts, I shall rest the assertion.

Mr. KING, the learned author of *Remarks concerning Stones said to have fallen from the Clouds, in these Days, and in ancient Times*, has adduced quotations of the greatest antiquity, descriptive of the descent of fallen stones; and, could it be thought necessary to add antique testimonies to those instanced by so profound an antiquarian, the quotations of Mons. FALCONET, in his papers upon Bœtilia, inserted in the *Histoire des Inscriptions et Belles-Lettres*;* the quotations in ZAHN's *Specula Physico-mathematica Historiana*;† the *Fisica Sotterranea* of GIACINTO GEMMA; the works of PLINY, and others, might be referred to.

* Tom. VI. P. 519. et Tom. XXIII. P. 228.

† Fol. 1696. Vol. I. p. 385. where a long enumeration of stones fallen from the sky is given.

Dr. CHLADNI, in his *Observations on the Mass of Iron found in Siberia, and on other Masses of the like Kind*, as well as in his *Observations on Fire-balls and hard Bodies fallen from the Atmosphere*, has collected almost every modern instance of phenomena of this nature.

Mr. SOUTHEY relates an account, juridically authenticated, of a stone weighing 10 lbs. which was heard to fall in Portugal, Feb. 19, 1796, and was taken, still warm, from the ground.*

The first of these peculiar substances with which chemistry has interfered, was the stone presented by the Abbé BACHELAY to the Royal French Academy. It was found on the 13th of September, 1768, yet hot, by persons who saw it fall. It is described as follows:

"La substance de cette pierre est d'un gris cendré pâle; "lorsqu'on en regarde le grain à la loupe, on apperçoit que "cette pierre est parsemée d'une infinité de petits points bril- "lans métalliques, d'un jaune pâle; sa surface extérieure, celle "qui, suivant M. l'Abbé BACHELAY, n'étoit point engagée dans "la terre, étoit couverte d'une petite couche très-mince d'une "matière noire, boursoufflée dans des endroits, et qui paroiss- "soit avoir été fondue. Cette pierre, frappée dans l'intérieur "avec l'acier, ne donnoit aucune étincelle; si on frappoit, au "contraire, sur la petite couche extérieure, qui paroissoit avoir "été attaquée par le feu, on parvenoit à en tirer quelques-unes."

The specific gravity of this stone was as 3535 to 1000.

The academicians analyzed the stone, and found it to contain,

Sulphur - - -	$8\frac{1}{2}$
Iron - - -	36
Vitrifiable earth - - -	$55\frac{1}{2}$
	100.

* Letters written during a short residence in Spain and Portugal. Page 239.

Of their mode of analysis, I shall have occasion to speak hereafter. They were induced to conclude, that the stone, presented to the Academy by the Abbé BACHELAY, did not owe its origin to thunder; that it did not fall from heaven; that it was not formed by mineral substances, fused by lightning; and that it was nothing but a species of pyrites, without peculiarity, except as to the hepatic smell disengaged from it by marine acid. "Que cette pierre, qui peut-être étoit couverte d'une "petite couche de terre ou de gazon, aura été frappée par la "foudre, et qu'elle aura été ainsi mise en évidence: la chaleur "aura été assez grande pour fondre la superficie de la partie "frappée, mais elle n'aura pas été assez long-tems continuée "pour pouvoir pénétrer dans l'intérieur; c'est ce qui fait que "la pierre n'a point été décomposée. La quantité de matières "métalliques qu'elle contenoit, en opposant moins de résistance "qu'un autre corps au courant de matière électrique, aura peut-"être pu contribuer même à déterminer la direction de la "foudre."

The Memoir is however concluded, by observing it to be sufficiently singular, that M. MORAND le Fils had presented a fragment of a stone, from the environs of Coutances, also said to have fallen from heaven, which only differed from that of the Abbé BACHELAY, because it did not exhale the hepatic smell with spirit of salt. Yet the academicians did not think any conclusion could be drawn from this resemblance, unless that the lightning had fallen by preference on pyritical matter.*

Mons. BARTHOLD, Professeur à l'Ecole centrale du Haut-Rhin, gave I believe the next, and last,† analytical account of

* See *Journal de Physique*. Tom. II. page 251.

† A very interesting detail of a meteor, and of stones fallen in July, 1790, was given by Professeur BAUDIN, in the *Magazin für das Neueste aus der Physik*, by Professor VOIGT.

what he also denominates *Pierre de Tonnerre*. He describes it thus: "La masse de pierre connue sous le nom de Pierre de "Tonnerre d'Ensisheim, pesant environ deux quintaux, a la "forme extérieure arrondie, presque ovale, raboteuse, d'un "aspect terne et terreux.

"Le fond de la pierre est d'une couleur grise bleuâtre, par-"semée de cristaux de pyrites, isolés, d'une cristalisation "confuse, en quelques endroits écailleuses, ramassés, formant "des nœuds et des petites veines, qui le parcourent en tout "sens: la couleur des pyrites est dorée; le poli leur donne un "éclat d'acier, et, exposées à l'atmosphère, elles ternissent et "brunissent. On distingue de plus, à l'œil nud, de la mine "de fer grise, écailleuse, non sulfureuse, attirable à l'aimant, "dissoluble dans les acides, peu oxidé, ou s'approchant beau-"coup de l'état métallique.

"La cassure est irrégulière, grenue, d'un grain un peu "serré: dans l'intérieur on voit de très petites fentes. Elle ne "fait pas feu au briquet; sa contexture est si lâche qu'elle se "laisse entamer au couteau. En la pilant, elle se réduit assez "facilement en une poudre grise bleuâtre, d'une odeur terreuse. "Quelquefois il se trouve des petits cristaux de mine de fer, "qui résistent plus aux coups du pilon."

The specific gravity of the piece in Professor BARTHOLD's possession, was 3233, distilled water being taken at 1000.

The analysis of Mons. BARTHOLD, of which I shall also have occasion to speak hereafter, gave in the 100,

Sulphur	2
Iron	20
Magnesia	14
Alumina	17
Lime	2
Silica	42
	97.

From the external characters, and from his analysis, the Professor considers the stone of Ensisheim to be argillo-ferruginous: and is of opinion that ignorance and superstition have attributed to it a miraculous existence, at variance with the first notions of natural philosophy.*

The account next in succession is already printed in the Transactions of the Royal Society; but cannot be omitted, as it immediately relates to one of the substances I have examined. I allude to the letter received by Sir WILLIAM HAMILTON, from the Earl of BRISTOL, dated from Sienna, July 12th, 1794. "In "the midst of a most violent thunder-storm, about a dozen "stones, of various weights and dimensions, fell at the feet of "different persons, men, women, and children. The stones are "of a quality not found in any part of the Siennese territory; "they fell about eighteen hours after the enormous eruption of "Mount Vesuvius; which circumstance leaves a choice of dif-"ficulties in the solution of this extraordinary phenomenon. "Either these stones have been generated in this igneous mass "of clouds, which produced such unusual thunder; or, which is "equally incredible, they were thrown from Vesuvius, at a "distance of at least 250 miles; judge then of its parabola. "The philosophers here incline to the first solution. I wish "much, Sir, to know your sentiments. My first objection was "to the fact itself; but of this there are so many eye witnesses, "it seems impossible to withstand their evidence." (Phil. Trans. for 1795. p. 103.) Sir WILLIAM HAMILTON, it seems, also received a piece of one of the largest stones, which weighed upwards of five pounds; and had seen another, which weighed about one. He likewise observed, that the outside of every stone which had been found, and had been ascertained to have fallen

* See *Journal de Physique, Ventose, An* 8. p. 169.

from the clouds near Sienna, was evidently freshly vitrified, and was black, having every sign of having passed through an extreme heat; the inside was of a light gray colour, mixed with black spots and some shining particles, which the learned there had decided to be pyrites.

In 1796, a stone weighing 56lbs. was exhibited in London, with several attestations of persons who, on the 13th of December, 1795, saw it fall, near Wold Cottage, in Yorkshire, at about three o'clock in the afternoon. It had penetrated through 12 inches of soil and 6 inches of solid chalk rock; and, in burying itself, had thrown up an immense quantity of earth, to a great distance: as it fell, a number of explosions were heard, about as loud as pistols. In the adjacent villages, the sounds heard were taken for guns at sea; but, at two adjoining villages, were so distinct of something singular passing through the air, towards the habitation of Mr. TOPHAM, that five or six people came up, to see if any thing extraordinary had happened to his house or grounds. When the stone was extracted, it was warm, smoked, and smelt very strongly of sulphur. Its course, as far as could be collected from different accounts, was from the south-west. The day was mild and hazy, a sort of weather very frequent in the Wold hills, when there are no winds or storms; but there was not any thunder or lightning the whole day. No such stone is known in the country. There was no eruption in the earth; and, from its form, it could not come from any building; and, as the day was not tempestuous, it did not seem probable that it could have been forced from any rocks, the nearest of which are those of Hamborough Head, at a distance of twelve miles.* The nearest volcano, I believe to be Hecla, in Iceland.

* Extracted from the printed paper delivered at the place of exhibition.

The exhibition of this stone, as a sort of show, did not tend to accredit the account of its descent, delivered in a hand-bill at the place of exhibition; much less could it contribute to remove the objections made to the fall of the stones presented to the Royal French Academy. But the Right Hon. President of the Royal Society, ever alive to the interest and promotion of science, observing the stone so exhibited to resemble a stone sent to him as one of those fallen at Sienna, could not be misled by prejudice: he obtained a piece of this extraordinary mass, and collected many references to descriptions of similar phenomena. At length, in 1799, an account of stones fallen in the East Indies was sent to the President, by JOHN LLOYD WILLIAMS, Esq. which, by its unquestionable authenticity, and by the striking resemblance it bears to other accounts of fallen stones, must remove all prejudice. Mr. WILLIAMS has since drawn up the following more detailed narrative of facts.

Account of the Explosion of a Meteor, near Benares, *in the* East Indies; *and of the falling of some Stones at the same Time, about* 14 *Miles from that City. By* John Lloyd Williams, *Esq. F. R. S.*

A circumstance of so extraordinary a nature as the fall of stones from the heavens, could not fail to excite the wonder, and attract the attention, of every inquisitive mind.

Among a superstitious people, any preternatural appearance is viewed with silent awe and reverence; attributing the causes to the will of the Supreme Being, they do not presume to judge the means by which they were produced, nor the purposes for which they were ordered; and we are naturally led to suspect the influence of prejudice and superstition, in their descriptions

of such phenomena; my inquiries were therefore chiefly directed to the Europeans, who were but thinly dispersed about that part of the country.

The information I obtained was, that on the 19th of December, 1798, about eight o'clock in the evening, a very luminous meteor was observed in the heavens, by the inhabitants of Benares and the parts adjacent, in the form of a large ball of fire; that it was accompanied by a loud noise, resembling thunder; and that a number of stones were said to have fallen from it, near Krakhut, a village on the north side of the river Goomty, about 14 miles from the city of Benares.

The meteor appeared in the western part of the hemisphere, and was but a short time visible: it was observed by several Europeans, as well as natives, in different parts of the country.

In the neighbourhood of Juanpoor, about 12 miles from the spot where the stones are said to have fallen, it was very distinctly observed by several European gentlemen and ladies; who described it as a large ball of fire, accompanied with a loud rumbling noise, not unlike an ill discharged platoon of musquetry. It was also seen, and the noise heard, by various persons at Benares. Mr. DAVIS observed the light come into the room where he was, through a glass window, so strongly as to project shadows, from the bars between the panes, on a dark coloured carpet, very distinctly; and it appeared to him as luminous as the brightest moonlight.

When an account of the fall of the stones reached Benares, Mr. DAVIS, the judge and magistrate of the district, sent an intelligent person to make inquiry on the spot. When the person arrived at the village near which the stones were said to have fallen, the natives, in answer to his inquiries, told him, that they

had either broken to pieces, or given away to the Tesseldar (native collector) and others, all that they had picked up; but that he might easily find some in the adjacent fields, where they would be readily discovered, (the crops being then not above two or three inches above the ground,) by observing where the earth appeared recently turned up. Following these directions, he found four, which he brought to Mr. DAVIS: most of these, the force of the fall had buried, according to a measure he produced, about six inches deep, in fields which seemed to have been recently watered; and it appeared, from the man's description, that they must have lain at the distance of about a hundred yards from each other.

What he further learnt from the inhabitants of the village, concerning the phenomenon, was, that about eight o'clock in the evening, when retired to their habitations, they observed a very bright light, proceeding as from the sky, accompanied with a loud clap of thunder, which was immediately followed by the noise of heavy bodies falling in the vicinity. Uncertain whether some of their deities might not have been concerned in this occurrence, they did not venture out to inquire into it until the next morning; when the first circumstance which attracted their attention was, the appearance of the earth being turned up in different parts of their fields, as before mentioned, where, on examining, they found the stones.

The assistant to the collector of the district, Mr. ERSKINE, a very intelligent young gentleman, on seeing one of the stones, brought to him by the native superintendant of the collections, was also induced to send a person to that part of the country, to make inquiry; who returned with several of the stones, and brought an account similar to that given by the person sent by Mr. DAVIS, together with a confirmation of it from the Cauzy,

(who had been directed to make the inquiry,) under his hand and seal.

Mr. MACLANE, a gentleman who resided very near the village of Krakhut, gave me part of a stone that had been brought to him the morning after the appearance of the phenomenon, by the watchman who was on duty at his house; this, he said, had fallen through the top of his hut, which was close by, and buried itself several inches in the floor, which was of consolidated earth. The stone must, by his account, previous to its having been broken, have weighed upwards of two pounds.

At the time the meteor appeared, the sky was perfectly serene; not the smallest vestige of a cloud had been seen since the 11th of the month, nor were any observed for many days after.

Of these stones, I have seen eight, nearly perfect, besides parts of several others, which had been broken by the possessors, to distribute among their friends. The form of the more perfect ones, appeared to be that of an irregular cube, rounded off at the edges; but the angles were to be observed on most of them. They were of various sizes, from about three to upwards of four inches in their largest diameter; one of them, measuring four inches and a quarter, weighed two pounds twelve ounces. In appearance, they were exactly similar: externally, they were covered with a hard black coat or incrustation, which in some parts had the appearance of varnish, or bitumen; and, on most of them were fractures, which, from their being covered with a matter similar to that of the coat, seemed to have been made in the fall, by the stones striking against each other, and to have passed through some medium, probably an intense heat, previous to their reaching the earth. Internally, they consisted of a number of small spherical bodies, of a slate colour, embedded

in a whitish gritty substance, interspersed with bright shining spiculæ, of a metallic or pyritical nature. The spherical bodies were much harder than the rest of the stone: the white gritty part readily crumbled, on being rubbed with a hard body; and, on being broken, a quantity of it attached itself to the magnet, but more particularly the outside coat or crust, which appeared almost wholly attractable by it.

As two of the more perfect stones which I had obtained, as well as parts of some others, have been examined by several gentlemen well versed in mineralogy and chemistry, I shall not attempt any further description of their constituent parts; nor shall I offer any conjecture respecting the formation of such singular productions, or even record those which I have heard of others, but leave the world to draw their own inferences from the facts above related. I shall only observe, that it is well known there are no volcanos on the continent of India; and, as far as I can learn, no stones have been met with in the earth, in that part of the world, which bear the smallest resemblance to those above described.

It remains for me to speak of a substance mentioned in the *Lithophylacium Bornianum*, Part I. page 125, described thus: "Ferrum retractorium, granulis nitentibus, matrice virescenti "immixtis, (*Ferrum virens* LINN.) cujus fragmenta, ab unius "ad vigenti usque librarum pondus, cortice nigro scoriaceo "circumdata, ad Plann, prope Tabor, circuli Bechinensis Bohe- "miæ, passim reperiuntur."

The iron thus described, is moreover made remarkable by a

note,* which observes, that credulous people assert it to have fallen from heaven, during a thunder storm, on the 3d of July, 1753.

The collection of Baron BORN, it is well known, has a place in the cabinet of the Right Hon. CHARLES GREVILLE, who, from the effect produced by comparing the histories and structure of the Italian and Yorkshire stones with the description of this iron, was induced to search the collection of BORN, where he discovered the very substance asserted to have fallen on the 3d of July, 1753. How far these four substances have resemblance to each other, it will soon appear not to be my province to anticipate.

The President having done me the honour to submit his specimens of the Yorkshire and Italian stones to my examination, I became indebted to Mr. GREVILLE and Mr. WILLIAMS for a similar distinction: and, being thus possessed of four substances, to all of which the same origin had been attributed, the necessity of describing them mineralogically did not fail to present itself. To execute this task, no one could be more eager, and certainly no one better qualified, than the Count de BOURNON. He has very obligingly favoured me with the following descriptions.

Mineralogical Description of the various Stones said to have fallen upon the Earth. By the Count de Bournon, *F. R. S.*

The stones I am about to describe, are not of any regular shape; and those which were found in an entire state, that is, those which had not been broken, either by their fall or other-

* Quæ (fragmenta) 3 Julii, anni 1753, inter tonitrua, e cœlo pluisse crediliores quidam asserunt.

wise, were entirely covered with a black crust, the thickness of which was very inconsiderable.

The stones which fell at Benares, are those of which the mineralogical characters are the most striking: I shall therefore begin the following description with them; and shall afterwards make use of them, as objects of comparison, in describing the others.

STONES FROM BENARES.

These stones, as well as the others described in this Paper, whatever may be their size, are covered over the whole extent of their surface, with a thin crust, of a deep black colour: they have not the smallest gloss; and their surface is sprinkled over with small asperities, which cause it to feel, in some measure, like shagreen, or fish skin.

When these stones are broken, so as to shew their internal appearance, they are found to be of a grayish ash colour; and of a granulated texture, very similar to that of a coarse grit-stone: they appear evidently to be composed of four different substances, which may be easily distinguished, by making use of a lens.

One of these substances, which is in great abundance, appears in the form of small bodies, some of which are perfectly globular, others rather elongated or elliptical. They are of various sizes, from that of a small pin's head to that of a pea, or nearly so: some of them, however, but very few, are of a larger size. The colour of these small globules is gray, sometimes inclining very much to brown: and they are completely opaque. They may, with great ease, be broken in all directions: their fracture is conchoid, and shews a fine, smooth, compact

grain, having a small degree of lustre, resembling in some measure that of enamel. Their hardness is such, that, being rubbed upon glass, they act upon it in a slight degree; this action is sufficient to take off its polish, but not to cut it: they give faint sparks, when struck with steel.

Another of these substances, is a martial pyrites, of an indeterminate form: its colour is a reddish yellow, slightly inclining to the colour of nickel, or to that of artificial pyrites. The texture of this substance is granulated, and not very strongly connected: when powdered, it is of a black colour. This pyrites is not attractable by the magnet; and is irregularly distributed through the substance of the stone.

The third of these substances consists in small particles of iron, in a perfectly metallic state, so that they may easily be flattened or extended, by means of a hammer. These particles give to the whole mass of the stone, the property of being attractable by the magnet; they are, however, in less proportion than those of pyrites just mentioned. When a piece of the stone was powdered, and the particles of iron separated from it, as accurately as possible, by means of a magnet, they appeared to compose about $\frac{3}{100}$ of the whole weight of the stone.

The three substances just described, are united together by means of a fourth, which is nearly of an earthy consistence. For this reason, it is easy to separate, with the point of a knife, or even with the nail, the little globular bodies above mentioned, or any other of the constituent parts of the stone we may wish to obtain. Indeed the stone itself may readily be broken, merely by the action of the fingers. The colour of this fourth substance, which serves as a kind of cement to unite the others, is a whitish gray.

The black crust with which the surface of the stone is coated, although it is of no great thickness, emits bright sparks, when struck with steel: it may be broken by a stroke with a hammer; and seems to possess the same properties as the very attractable black oxide of iron. This crust is, however, like the substance of the stone, here and there mixed with small particles of iron in the metallic state: they may easily be made visible, by passing a file over the crust, as they then become evident, on account of their metallic lustre. This is more particularly the case with respect to the crust of those stones which remain to be mentioned, they being much more rich in iron than that I have just described; a circumstance I think it needless to repeat, in the following descriptions of them. The stone now treated of, does not, when breathed upon, emit an argillaceous smell: the same remark may be applied to all the others.

The specific gravity of this stone is 3352.

STONE FROM YORKSHIRE.

This stone, the constituent parts of which are exactly the same as those of the stones from Benares, differs from them, however,

First. In having a finer grain.

Secondly. That the substance described as being in the form of small globular or elliptical bodies, is not so constantly in those forms, but is also found in particles of an irregular shape; a circumstance that is not met with in the other stones: these bodies are likewise, in general, of a smaller size.

Thirdly. The proportion of martial pyrites, which has precisely the same characters as that in the stones from Benares, is less; on the contrary, that of the iron in a metallic state, is much greater. The quantity I was able to separate by means

of the magnet, appeared to me to compose about eight or nine parts, in one hundred, of the weight of the whole mass. I observed many pieces of this iron, of a pretty considerable size; one of them, taken from a portion of the stone I had powdered, in order to separate the iron, weighed several grains.

The part of the stone which is in an earthy state, and which serves to connect the other parts together, has rather more consistence than that of the preceding stones; and its appearance does not differ much from that of decomposed felspar or kaolin. The stone itself, therefore, although by no means hard, is rather more difficult to break with the fingers.

The specific gravity of this stone is 3508.

STONE FROM ITALY.

This stone was in a perfectly entire state; consequently, its whole surface was covered over with the black crust peculiar to all stones of this kind. As the stone was of a very small size, it became necessary to sacrifice the whole of it to the investigation of its nature. Its grain was coarse, similar to that of the stones from Benares: in it might be perceived the same gray globular bodies, the same kind of martial pyrites, and the same particles of iron in the metallic state. The proportion of these last was much less than in the stone from Yorkshire; but rather greater than in the stones from Benares. The same kind of gray earthy substance served to connect the different parts together; and nothing more could be perceived, except a few globules, which consisted wholly of black oxide of iron, attractable by the magnet, and one single globule of another substance, which appeared to differ from all those we have already described. This last substance had a perfectly vitreous lustre, and was completely transparent: it was of a pale yellow

colour, slightly inclining to green; and its hardness was rather inferior to that of calcareous spar. The quantity of it, however, was too small to be submitted to such an investigation as might have determined its nature. The black crust which covered the stone, was rather thinner than that of the stones already described; and seemed to have undergone a kind of contraction, which had produced in it a number of fissures or furrows, thereby tracing upon the surface the appearance of compartments, similar in some measure to what is observed in the stones called Septaria.

The specific gravity of this stone was 3418.

STONE FROM BOHEMIA.

The internal structure of this stone is very similar to that of the stone from Yorkshire. Its grain is finer than that of the stones from Benares: in it may be observed the same gray substance, both in small globules and in particles of an irregular shape; also the same particles of metallic iron. The same kind of earthy substance likewise served to connect the other parts together.

This stone, however, differs materially from the others.

First. The particles of pyrites cannot be seen without a lens.

Secondly. It contains a much larger quantity of iron in the metallic state; insomuch, that the proportion of that metal, separated from it by means of the magnet, amounted to about $\frac{25}{100}$ of the weight of the whole.

This stone has also (owing perhaps to its having remained a much longer time in the earth than the preceding ones, all of which were taken up nearly at the very instant of their fall,) another difference, viz. many of the particles of iron in a

metallic state, have undergone an oxidizement at their surface; a circumstance that has produced a great number of spots, of a yellowish brown colour, and very near to each other, over a part of its internal substance. This oxidizement, by adding to the bulk, and to the force of action, of the part we have described as serving by way of cement to the other constituent parts of the stone, has occasioned a greater degree of adhesion between these parts, and has rendered the substance of the stone more compact.

The great quantity of iron in a metallic state which this stone contains, added to its greater compactness, makes it capable of receiving a slight degree of polish; whereas it is impossible to give any polish to the others. When polished, the iron becomes very evident, in the polished part; appearing in the form of small specks, almost close to each other, which have the colour and lustre peculiar to that metal: these specks are, in general, nearly of an equal size.

The black crust of this stone is similar to that of the others.

The specific gravity of the stone is 4281.

It is easy to perceive, from the foregoing description, that these stones, although they have not the smallest analogy with any of the mineral substances already known, either of a volcanic or any other nature, have a very peculiar and striking analogy with each other. This circumstance renders them truly worthy to engage the attention of philosophers; and naturally excites a desire of knowing to what causes they owe their existence.

I proceed to consider the assistance to be derived from chemistry, in distinguishing these stones from all other known

substances, and in establishing the assertion, that they have fallen on the earth.

The analysis made by the French Academicians, of the stone presented to them by the Abbé BACHELAY, was, in part, conducted by the ever to be deplored LAVOISIER; but it was performed before that celebrated author had enriched chemistry with his last discoveries, and before he had given birth to the system under which it flourishes. The result of this analysis might well induce the conclusion, that the subject of it was common pyritical matter. It was unfortunately made of an aggregate portion of the stone, and not of each distinct substance, irregularly disseminated through it. The proportions obtained were, consequently, as accidental as the arrangement of every substance in the mass.

The analysis of M. BARTHOLD, of the stone of Ensisheim, is subject to the same objections: but, after having the advantage of the foregoing descriptions, the researches which follow cannot be supposed altogether liable to a similar fatality.

EXAMINATION OF THE STONE FROM BENARES.

This stone, as the Count de BOURNON has already remarked, has the most distinguished characters. Indeed it is the only one of the four, sufficiently perfect (if I be allowed that expression) to be subjected to any thing approaching to a regular analysis.

The crust, or external black covering, is the first substance to which the attention is naturally directed. When a portion of this crust had been detached with a knife, or a file, and finely pulverized, I separated the particles attractable by a magnet;

and digested the unattractable portion with nitric acid, which was presently decomposed; but, owing to a strong adherence of some of the interior and earthy parts of the stone, it did not disentangle the coating or metalline part without some difficulty. The acid being sufficiently neutralized, the solution was passed through a filtre, and saturated to excess with ammonia. An abundant precipitate of oxide of iron was produced; and, when this oxide was separated, I observed the saline liquor to have a greenish colour. I evaporated it to dryness; and redissolved the dry salt in distilled water. No precipitate was formed during the evaporation, nor was the colour of the solution entirely destroyed. It appeared to me like a triple salt, described by Mr. HERMSTADT* as an ammoniacal nitrate of nickel. By examination with prussiate of ammonia, it yielded a whitish precipitate, inclining to a violet colour; and, by various properties, I was soon confirmed in the opinion, that nickel was present. Since I shall have occasion more than once to treat of the triple compound, and since it has been only mentioned by Mr. HERMSTADT, it is necessary now to detail some of its distinctive characters. The same chemist informs us, that the three mineral acids, with ammonia, enter into similar combinations with nickel; and I have observed, that oxide of nickel can be dissolved by nitrate and muriate of ammonia. The muriate seems to take up the largest quantity. The colour of this salt is by no means uniform: it is sometimes grass green, violet, rose colour, inclining to purple, and I have seen it almost colourless. It seems to be purple, and to incline to rose colour and violet, when all the oxide of nickel is not united to both acid and alkali, but, from the deficiency of salt, is held in solution by an

* *Annales de Chimie.* Tom. XXII. p. 108.

excess of ammonia. In this case, evaporation, of course, precipitates the nickel in the state of oxide, which is of a whitish green colour.

The nickel cannot be *precipitated* from a perfectly formed triple salt, by any reagent I have tried, except by a prussiate, or a hydrogenized sulphuret of ammonia. Potash and lime, as well as, I presume, other bodies, standing in the order of affinities before ammonia, decompose the salt; but the nickel is then continued in solution by the disengaged ammonia.

As it may be imagined that I have occasionally met with copper, when I describe a violet or purple ammoniacal solution, it is right to observe, that to avoid this error, I have either reduced the liquor to a neutral state, and endeavoured, without success, to obtain from it a precipitate, with a solution of sulphureted hydrogen gas; or, by adding an acid to slight excess, and immersing a piece of iron, I have not been able to detect a trace of copper. These, and many other trials, when they do not appear to be made before the estimation of the quantities of nickel, have been constantly made afterwards.

But, to return to the incrustation or coating of the stone, the decomposition of the nitric acid shewed the presence of matter at least nearly metallic, although not attractable; and the examinations made of the liquor, from which the iron was precipitated, ascertained the presence of nickel beyond dispute. The difficulty of obtaining the coating of the stone, either distinct from matter not belonging to it, or in sufficient quantity, induced me to relinquish the idea of attempting to give the proportions of its constituent parts.

The stone being deprived of its covering, the shining particles irregularly disseminated, next demand examination. I first

examined the pyrites. Their very loose texture made it exceedingly difficult to collect the weight of 16 grains, which was however effected by the dexterity of the Count de BOURNON.

I digested these, at a low heat, with weak muriatic acid; which acted gradually, and disengaged a trifling but sensible quantity of sulphureted hydrogen gas. After several hours, I found the acid discontinued its action. The whole metalline part appeared in solution; but sulphur and earthy particles were observable. The sulphur, from its small specific gravity, was suspended through the solution; whilst the earthy matter, which could not be separated by mechanical means, was fortunately left at the bottom of the digesting vessel. I decanted off the solution, holding suspended the sulphur; and, by repeated washing, separated every thing belonging to the pyrites from the insoluble earthy matter, the subtraction of which reduced the weight of real pyrites to 14 grains. I next obtained the sulphur, by filtration. When it was as dry as I could make it, without fear of its being sublimed, its weight was two grains. To the filtrated liquor I added nitrate of barytes, by way of detecting any sulphuric acid which might have been present; but no cloudiness ensued. I then separated, by sulphate of ammonia, the barytes thus added, and precipitated the iron with ammonia. The liquor, on the subsidence of oxide of iron, appeared of a violet purple colour: it contained nickel, which I threw down with sulphureted hydrogen gas, there being already a sufficient excess of ammonia in the saline liquor to form an alkaline hydrogenized sulphuret. The oxide of iron, after ignition, weighed 15 grains; and the sulphuret of nickel, reduced to an oxide, weighed, after the same treatment, something more than one grain. The proportions of the substances contained in the

pyrites of the stone from Benares, may therefore be considered nearly thus:

	Grains.
Sulphur - - - - - -	2
Iron - - - - - -	10½

Since 15 grains of the oxide represent about that quantity of iron,

Nickel, nearly - - - -	1
Extraneous earthy matter - - -	2
	15½.

It is observable that, notwithstanding the loss appears to be only half a grain, it was probably more, because the sulphur could not be reduced to the same state of dryness in which it existed when in combination with the iron; not to say that it was, in a small degree, volatilized with the hydrogen gas disengaged during the solution.

The weight of nickel is a mere estimation. We are not yet sufficiently acquainted with that metal to speak of it with accuracy, except as to its presence. Upon the whole, however, it may be concluded, that these pyrites are of a very particular nature; for, although HENKEL has observed that sulphur may be separated from pyrites by muriatic acid, it is by no means the usual habitude of pyrites to be of such easy decomposition.

The other shining particles immediately seen, when the internal structure of the stone is exposed, are the malleable iron. Before I state the examination of this iron, I must remark, that preliminary experiments having shewn me it contained nickel, I treated several kinds of the most pure irons I could obtain, with nitric acid; and precipitated the oxide from the metallic salt by ammonia. The quantity of oxide I obtained from 100 grains of iron, was from 144 to 146. I may consequently

infer, that 100 grains of pure iron acquires, by such a process, 45 grains of oxygen; and that, whenever a metallic substance, supposed to be iron, does not, under the same circumstances, acquire the same proportionate weight, something is either volatilized, or left in solution. Hence, when a metallic alloy of nickel and iron presents itself, a judgment may, at least, be formed of the quantity of nickel, by the deficiency of weight in the precipitated oxide of iron.

This mode of treatment was not allowed me in the examination of the coating of the stone, because it was impossible to know in what state of oxidizement the iron existed. But, as the particles disseminated through the whole mass, are clearly metallic, a very tolerable idea of the quantities of nickel contained in them will be obtained, by noting the quantity of oxide of iron separated, as above described. 25 grains of these metallic particles were therefore heated with a quantity of nitric acid, much more than sufficient to dissolve the whole. Some earthy matter, which, as in a former case, was not separable by mechanical means, remained after a complete solution of the metal had been effected. This earthy matter, after being ignited, weighed two grains. The real matter of the present examination, was therefore reduced to 23 grains, and was in complete solution. I added ammonia to a very sensible excess. The oxide of iron was thereby precipitated, and, being collected and ignited, it weighed 24 grains; whereas, according to my experiments, $33\frac{1}{2}$ grains should have been produced from the solution, had it contained nothing but iron. I examined the saline liquor, when free from ferruginous particles, and discovered it to be the triple salt of nickel. Hence, allowing for loss, the quantity of nickel may be estimated, by calculating the quantity of iron contained

in 24 grains of oxide. Thus, if 145 grains of oxide contain 100 of iron, about $16\frac{1}{2}$ are contained in 24 of oxide. This would suppose the 23 grains of alloy to consist of $16\frac{1}{2}$ iron and $6\frac{1}{2}$ nickel; which, if the usual loss be added to the $16\frac{1}{2}$ grains of iron, and deducted from the nickel, may not be very remote from the truth.

I shall next examine the globular bodies, also irregularly dispersed throughout the stone. A number of them were reduced to fine powder; but nothing metallic could be separated by the magnet. As a preliminary experiment, I sought for pyrites, by digestion with muriatic acid; but no hepatic smell was in the least perceivable, nor was white carbonate of lead at all altered by being held over the mixture. I therefore conclude these globular bodies do not envelope either iron or pyrites. By way of analysis, I treated 100 grains with potash, in a silver crucible; and, after the usual application of a red heat, separated as much silica as possible, by muriatic acid and evaporation. The silica being collected on a filtre, carbonate of potash was added to the filtrated liquor; by which, a precipitate, almost wholly ferruginous, was produced. This precipitate was collected in the common way; then boiled with potash, to extract alumina; and, by supersaturating the alkaline liquor with muriatic acid, and precipitating by carbonate of ammonia, an earth was gathered, which I afterwards found to be partly, if not entirely, siliceous. After redissolving, in muriatic acid, the portion of the ferruginous matter rejected by the potash, I precipitated by ammonia, what I took to be entirely oxide of iron; but, after igniting it, and again attempting to redissolve the whole in muriatic acid, more silica was left. The non-existence of lime was proved, by the addition of carbonate of ammonia,

immediately after the same alkali, pure, had thrown down what I took wholly for oxide of iron. I had now obtained every thing in the subject of my analysis, except magnesia and nickel. The former, and a trace of the latter, were held by carbonic acid in the liquor, from which the ferruginous precipitate was, in the first instance, thrown down by carbonate of potash; and the latter was found in the last named muriate of ammonia. I disengaged the magnesia, by the assistance of potash, and by evaporating to dryness. The oxide of nickel was precipitated by hydrogenized sulphuret of ammonia.

Under all circumstances, I am induced to state the proportions of constituent parts thus:

Silica - - - -	50
Magnesia - - -	15
Oxide of iron - - -	34
Oxide of nickel - - -	$2\frac{1}{2}$
	$101\frac{1}{2}$.

The excess of weight, instead of the usual loss, is owing to the difference of oxidizement of the iron, in the stone and in the result of the analysis; which will be found to be the case in all analyses of these substances; indeed it is always necessary to reduce the oxide to the red state, as being the only one to be depended upon. To avoid future repetition, I shall also observe, first, that by preliminary experiments, I could not detect any other substance than those mentioned. Secondly, that the earth obtained as alumina, appeared to me to be mostly, if not entirely, siliceous; because, after it had been ignited, and again treated with potash and muriatic acid, I found it was very nearly all precipitated by evaporation. Thirdly, I examined, and judged of, the silica collected from the oxide of iron, in the

same way. Fourthly, the weight of the magnesia is given, not immediately, as obtained by evaporation, but after a subsequent solution in an acid, and precipitation by potash. And, fifthly, the proportions are taken from the mean of two analyses.

Nothing remains to be examined, of the stone from Benares, except the earthy matter, forming a cement or matrix for the substances already examined. 100 grains of this matter were, by mechanical means, separated as perfectly as possible, from the pyrites, iron, and globular bodies, and analysed as above. The mean result of two analyses gave,

Silica - - - -	48
Magnesia - - - -	18
Oxide of iron - - -	34
Oxide of nickel - - -	2½
	102½.

EXAMINATION OF THE STONE FROM SIENNA.

The external coating of this stone appeared to have the same characters as that of the stone from Benares.

The pyrites, although certainly present, were not crystallized in such groups as in the preceding stone; nor could they be separated by mechanical means.

The attractable metal was easily separated by the magnet; but 8½ grains only were collected. I treated them with nitric acid and ammonia, as in a preceding case. Nearly one grain of earthy matter was insoluble; the weight was therefore reduced to rather less than 8 grains. The oxide of iron, precipitated by ammonia, weighed 8 grains; and the saline liquor gave abundant indications of nickel. As 8 grains of this oxide of iron contain nearly 6 of metal, the quantity of nickel, in the bare 8

grains, may be estimated between 1 and 2 grains. Some globular bodies were extracted, but too few to analyze.

Since the pyrites could not be separated, I collected 150 grains of the stone, freed from iron by the magnet, and as exempt as possible from globular bodies. These 150 grains, I first digested with muriatic acid, that the pyrites might be decomposed, and every thing taken up which could be dissolved by that menstruum. A very decided disengagement of sulphureted hydrogen gas was occasioned. When the acid could produce no further action, I collected the undissolved matter on a filtre, and boiled it with the most concentrate nitric acid, in hopes of being able to convert the sulphur, previously liberated, into sulphuric acid; but my endeavours were fruitless; for, upon the addition of nitrate of barytes to the nitric solution, rendered previously transparent, a very insignificant quantity of sulphate of barytes was obtained. The surplus of barytic nitrate was removed by sulphate of potash. I next completely edulcorated the mass which remained insoluble, after the action of the muriatic and nitric acids; and, adding the water of edulcoration to the muriatic and nitric liquors, evaporated the whole for silica. I then submitted the mass, undissolved by the acids and the water, to the treatment with potash, muriatic acid, and evaporation, which was, in the first instance, applied to the stone from Benares. The first precipitation was, as in that analysis, also effected with carbonate of potash; but, instead of endeavouring immediately to extract alumina, I ignited the precipitate, that the alumina or silica remaining might be rendered insoluble. After the ignition, I separated the oxide of iron with very concentrate muriatic acid; and the earths, which were left perfectly white, I heated with potash, until they were

again capable of being taken up by the same acid. The solution so made, was slowly evaporated; and, as very nearly every thing was deposited during the evaporation, I conclude all was silica. The proportions resulting from this single analysis, without the weight of sulphur contained in the pyrites irregularly disseminated through the whole, were,

Silica	70
Magnesia	34
Oxide of iron	52
Oxide of nickel	3
	159.

EXAMINATION OF THE STONE FROM YORKSHIRE.

The mechanical separation of the substances in this stone being as difficult as in the preceding case, I was necessarily satisfied with submitting it to the same treatment. I collected, however, 34 grains of malleable particles; which, by the process already more than once mentioned, left 4 grains of earthy matter; and, by yielding $37\frac{1}{2}$ of oxide of iron, indicated about 4 grains of nickel.

150 grains of the earthy part of the stone were, by analysis, resolved into,

Silica	75
Magnesia	37
Oxide of iron	48
Oxide of nickel	2
	162.

EXAMINATION OF THE STONE FROM BOHEMIA.

The probability of never being able to obtain another specimen of the very remarkable fragment of this substance, did

not allow me to trespass more on the liberality of Mr. GREVILLE, than to detach a small portion. I found it of similar composition to that of the three preceding stones; and the Count de BOURNON has already shewn the proportionate quantity of the attractable metal to be very considerable. $16\frac{1}{2}$ grains, left $2\frac{1}{2}$ of extraneous earthy matter; and yielded, by the treatment with nitric acid and ammonia, $17\frac{1}{2}$ grains of oxide of iron. This would seem to induce an estimation of $1\frac{1}{2}$ of nickel in 14 grains, or about 9 per cent.

55 grains of the earthy part of the stone, by the analytical treatment of the two former, afforded,

Silica - - - -	25
Magnesia - - -	$9\frac{1}{2}$
Oxide of iron - - -	$23\frac{1}{2}$
Oxide of nickel - -	$1\frac{1}{2}$
	$59\frac{1}{2}$.

The unusual increase of weight in the result of the three last analyses, notwithstanding the entire loss of the sulphur in the pyrites, is obviously owing to the metallic state of the iron combined with the sulphur, as was shewn in a former instance.

I have now concluded the chemical examination of these four extraordinary substances. It unfortunately differs from the analysis made by the French Academicians, of the stone presented to them by the Abbé BACHELAY, as well as from that made by Professor BARTHOLD, of the stone of Ensisheim. It is at variance with that of the Academicians, inasmuch as they found neither magnesia nor nickel. It differs from that of Mr. BARTHOLD, as he did not find nickel, but discovered some lime, with 17 per cent. of alumina. With regard to these differences, I have to submit to the chemical world, whether magnesia might not

have eluded the action of an acid, when the aggregation of the integrant parts of the stone was not destroyed by treatment with potash. As to the existence of alumina, I do not absolutely deny it; yet I must observe, that the whole of the earth which seemed to have any resemblance, however small, to alumina, was at most 3 per cent. and there seems good reason to consider it as silica. Respecting the existence of lime in the stone of Ensisheim, I must appeal to Professor BARTHOLD, whether, supposing lime a constituent part, sulphate of lime should not have been formed, as well as sulphate of magnesia, when sulphuric acid was generated by igniting the earths and pyrites. And, as to the proportion of alumina, in the same stone, I would ask, at least, whether it would have been so considerable, if the solutions formed by acids, after the treatment with potash, had been evaporated to the requisite dryness: not to observe, that no mention is made of any examination of the properties of the earth called alumina. In the proportion of magnesia, I have the satisfaction to find my analysis correspond very nearly with that of Professor BARTHOLD; and, if what he considered alumina were supposed silica, the stone presented to the French Academy, the stone of Ensisheim, and the four I have examined, would agree very nearly in siliceous proportions. With respect to the nickel, I am confident it would have been found in all, had the metallic particles been separately examined. But, whatever be these variations, the mineralogical description of the French Academicians, of Mr. BARTHOLD, and of the Count de BOURNON, all exhibit a striking conformity of character, common to each of these stones; and I doubt not but the similarity of component parts, especially of the malleable alloy, together with the near approach of the

constituent proportions of the earths contained in each of the four stones, the immediate subject of this Paper, will establish very strong evidence in favour of the assertion, that they have fallen on our globe. They have been found at places very remote from each other, and at periods also sufficiently distant. The mineralogists who have examined them, agree that they have no resemblance to mineral substances, properly so called; nor have they been described by mineralogical authors. I would further urge the authenticity of accounts of fallen stones, and the similarity of circumstances attendant on such phenomena; but, to the impartial it would be superfluous, and, to those who disbelieve whatever they cannot explain, it would be fruitless. Attempts to reconcile occurrences of this nature with known principles of philosophy, it is true, are already abundant; but (as the Earl of BRISTOL has well expressed) they leave us a choice of difficulties equally perplexing. It is however remarkable, that Dr. CHLADNI, who seems to have indulged in these speculations with most success, should have connected the descent of fallen stones with meteors; and that, in the narrative of Mr. WILLIAMS, the descent of the stones near Benares, should have been immediately accompanied with a meteor.

No luminous appearance having been perceived during the day on which the stone fell in Yorkshire, it must be admitted, rather militates against the idea, that these stones are the substances which produce or convey the light of a meteor, or that a meteor must necessarily accompany them.* Yet the stones from Sienna fell amidst what was imagined lightning, but what might in reality have been a meteor. Stones were also found,

* In the account of the stone which fell in Portugal, no mention is made, either of a meteor or lightning.

after the meteor seen in Gascony, in July, 1790. And Mr. FALCONET, in the memoir I have already quoted, relates, that the stone which was adored as the mother of the gods, was a Bœtilia; and that it fell at the feet of the poet PINDAR, enveloped in a ball of fire. He also observes, that all the Bœtilia had the same origin.

I ought not perhaps to suppress, that in endeavouring to form an artificial black coating on the interior surface of one of the stones from Benares, by sending over it the electrical charge of about 37 square feet of glass, it was observed to become luminous, in the dark, for nearly a quarter of an hour; and that the tract of the electrical fluid was rendered black. I by no means wish to lay any stress upon this circumstance; for I am well aware, that many substances become luminous by electricity.

But, should it ever be discovered that fallen stones are actually the bodies of meteors, it would not appear so problematical, that such masses as these stones are sometimes represented, do not penetrate further into the earth: for meteors move more in a horizontal than in a perpendicular direction; and we are as absolutely unacquainted with the force which impels the meteor, as with the origin of the fallen stone.

Before I close this subject, I may be particularly expected to notice the meteor which, a few months ago, traversed the county of Suffolk. It was said, that part of it fell near Saint Edmundsbury, and even that it set fire to a cottage in that vicinity. It appeared, from inquiries made on the spot, that something, seemingly from the meteor, was, with a degree of reason, believed to have fallen in the adjacent meadows; but the time of the combustion of the house did not correspond with the moment of the meteor's transition. A phenomenon much

more worthy of attention, has since been described in the Philosophical Magazine. On the night of the 5th of April, 1800, a body wholly luminous, was seen, in America, to move with prodigious velocity. Its apparent size was that of a large house, 70 feet long; and its elevation above the surface of the earth, about 200 yards. The light produced effects little short of sunbeams; and a considerable degree of heat was felt by those who saw it, but no electric sensation. Immediately after it disappeared in the north-west, a violent rushing noise was heard, as if the phenomenon were bearing down the forest before it; and, in a few seconds after, there was a tremendous crash, causing a very sensible earthquake. Search being afterwards made in the place where the burning body fell, every vegetable was found burnt, or greatly scorched, and a considerable portion of the surface of the earth broken up. We have to lament, that the authors of this account did not search deeper than the surface of the ground. Such an immense body, though moving in a horizontal direction, could not but be buried to a considerable depth. Should it have been more than the semblance of a body of a peculiar nature, the lapse of ages may perhaps effect what has now been neglected; and its magnitude and solitary situation become the astonishment of future philosophers.

This leads me to speak of the solitary mass of what has been called native iron, which was discovered in South America, and has been described by Don RUBIN DE CELIS. Its weight was about 15 tons. The same author mentions another insulated mass of the same nature. The whole account is exceedingly interesting; but, being already published in the Philosophical Transactions for the year 1788, it needs not be here repeated.

Mr. PROUST has shewn the mass particularly described, not to

be wholly iron, but a mixture of nickel and iron. The Trustees of the British Museum, who are in possession of some fragments of this mass, sent to the Royal Society by Don RUBIN DE CELIS, have done me the honour to permit me to examine them; and I have great satisfaction in agreeing with a chemist so justly celebrated as Mr. PROUST.

The connexion which naturally exists between one mass of native iron and another, immediately turns our attention to the native iron in Siberia, described by PALLAS; and this, we are told, the Tartars considered as a sacred relic, which had dropped from heaven. The nickel found in the one mass, and the traditional history of the other, not to compare the globular bodies of the stone from Benares with the globular concavities and the earthy matter of the Siberian iron, tend to the formation of a chain between fallen stones and all kinds of native iron. How far any real affinity exists between these several substances, very obliging friends have afforded me an opportunity to form some judgment. I am indebted to Mr. GREVILLE and Mr. HATCHETT for portions of almost every known native iron: and the Count de BOURNON has done me the favour particularly to describe them as follows.

Description of various Kinds of native Iron. By the Count de Bournon.

The great number of particles of iron, in a perfectly metallic state, contained in the stone from Bohemia, and the said particles being so near each other, naturally lead to some reflections respecting the existence of native iron, which, by many mineralogists, is still considered as problematical. Let us suppose for a moment, that these particles of iron were to

approach still more nearly to each other, so as absolutely to come into contact, and in that manner to form a kind of chain, folded upon itself in the interior part of the substance, and leaving a great number of cavities between the links of the chain so folded. Let us then suppose, that the earthy substance with which these cavities are filled, being very porous, and having but a small degree of consistence, should (as may happen by a variety of causes) be destroyed. It is plain, that if such a destruction were to take place, the iron alone would remain; and, being thus left bare, it would appear in the form of a mass, more or less considerable, of a cellular texture, and as it were ramified; such a form, in short, as that in which most of the native irons we are acquainted with have been found. May it not be fair to attribute to such an origin, the native iron found in Bohemia, a specimen of which was presented by the Academy of Freyberg to Baron BORN, and which came, with the rest of his collection, into the hands of Mr. GREVILLE? May not such also, notwithstanding the enormity of its bulk, be the origin of the mass of native iron found in Siberia, near Mount Kemirs, by the celebrated PALLAS?

We have already seen, in the results of the analyses made by Mr. HOWARD, of the various stones above described, that he constantly found a certain proportion of nickel mixed with the iron they contained. This circumstance recalls to our notice the observations that were made by Mr. PROUST, some time ago, respecting the mixture of nickel in the native iron of South America; and tends to give some additional support to the opinion hinted at in the foregoing paragraph.

The circumstances just mentioned, naturally gave to Mr. HOWARD, as well as to me, a desire to know whether the

native iron from Siberia, and that from Bohemia, were also mixed with nickel. Mr. HOWARD, consequently, lost no time in proceeding upon this important investigation. The native iron of Siberia presents some very interesting peculiarities, and has often been referred to, but has not yet been properly described; it is therefore with great pleasure that I add the following description of it, and of some other kinds of native iron, to the description I have already given of the various stones said to have fallen on the earth.

I feel the greater satisfaction in doing this, as the noble collection of Mr. GREVILLE contains two specimens of this iron, in perfect condition; one of which weighs several pounds, and was sent to Mr. GREVILLE by Mr. PALLAS himself: on this account, therefore, I enjoy an advantage that many of the authors who have spoken of this iron probably wanted.

One of these pieces has a cellular and ramified texture, analogous to that of some very porous and light volcanic scoria: this is the usual texture of the specimens of this kind of iron, which are preserved in the various mineralogical collections in Europe. When it is attentively examined, there may be perceived in it, not only empty cells, but also impressions or cavities, of greater or less depth, and sometimes perfectly round, which appear evidently to be the result of the compression of hard bodies, which were situated there, and which, when they came away, left the surface of these cavities quite smooth, and having the lustre of polished metal. Here and there, in some of these cavities, there remains a transparent substance, of a yellowish green colour, of which I shall treat more particularly, when I come to the description of the second of the specimens above mentioned. It is very clear, that the cavities here spoken of

owe their existence to this transparent substance; and that the polish of the cavities arises merely from the compression of the said substance, and is the natural consequence of its surface having been in perfect contact with that of the iron.

This iron is very malleable: it may be easily cut with a knife; and may be as easily flattened or extended by means of a hammer. Its specific gravity is 6487; which, however, is very much under that of iron which has been merely melted, and has not been forged. The specific gravity of the native iron of Bohemia, which is nearly as malleable and as easy to be cut, is still less: I found it not to exceed 6146. This low degree of gravity, appears to be owing partly to the oxidizement of the surface of the iron, and partly to there being, in the interior part of its substance, a number of small cavities, which are often rendered visible by fracture, and which have their surfaces also oxidized. The fracture of this iron, presents the same shining and silvery white colour as the common cast iron, known by the name of white cast iron; but its grain is much smoother and finer: it is also much more malleable when cold. BERGMAN says that this iron is brittle, when heated to a red heat. I have frequently tried it in that state, and have constantly found it to be malleable. The same remark may be applied to the native iron from South America; and also to that from Senegal.

The second of the two specimens mentioned above, and which weighs several pounds, presents an aspect that differs, in some respects, from that of the preceding specimen. The most considerable part of it forms a solid compact mass, in which there is not to be perceived the smallest appearance of pores or cavities; but there arises upon its surface, a kind of ramified

or cellular part, similar, in every respect, to the specimen already described, and every where completely connected with the substance of the mass itself.

If the compact part of this piece is examined with attention, it will be perceived, that it is not entirely composed of iron in the metallic state, but that it is mixed with nearly an equal quantity of the transparent substance of a yellowish green colour, (sometimes also of a greenish yellow,) already spoken of in the description of the other specimen. This substance is mixed with the iron, in such a manner, that if the whole of the former could be removed, the remaining part would consist merely of iron in the metallic state, and would present the same cellular appearance as the preceding specimen, and the ramified or cellular part of the specimen now described.

This stony part, separated from the iron, appears in the form of small nodules, generally of an irregular shape, but sometimes nearly globular: they have a perfectly smooth and shining surface, so as very often to present the appearance of small balls of glass; a circumstance that has led many persons to suppose them the result of a real vitrification. Some of these nodules have several irregular facets, produced by the compression of the iron in which they were inclosed; but I have never observed in them, any appearances that could lead me to suspect they had the slightest tendency whatever to assume a determinate crystalline form.

This substance is always more or less transparent. It is sufficiently hard to cut glass; but has no effect upon quartz. It is very brittle: its fracture is usually conchoid; but I could not perceive that it broke in any particular direction, in such a way that I could consider the fracture as a natural one. It becomes

electric by friction. Its specific gravity is from 3263 to 3300. It is very refractory: I kept it, for some time, exposed to a degree of heat sufficiently strong to oxidize, to a considerable depth, the iron crucible in which it was placed, without its having undergone any alteration, except that of having acquired a greater degree of intensity in its colour. Its transparency was not at all diminished. I think, therefore, there is not the smallest reason to allow any probability to the opinion that it ought to be considered as a kind of glass.

Of all substances hitherto known, that with which it seems to have the greatest analogy, is the peridot, (the chrysolite of WERNER,) to which some mineralogists have referred it. The result of Mr. HOWARD's analysis of it, is nearly the same as that of the analysis of the peridot, made by Mr. KLAPROTH.

The hardness and infusibility of this substance are nearly the same as those of the peridot; but it seems to have a rather less degree of specific gravity: that of two very perfect crystals of peridot, I found to be from 3340 to 3375. The crystalline forms of the substance here described, if ever we should be able to determine them, would clear up our doubts respecting the analogy between the two substances. If we consider the compact part of the specimen now treated of, particularly the strong connexion that appears to exist between the iron and the transparent substance, and the great resistance we experience when we attempt to separate them, we cannot help being surprised, that almost all the specimens of this mass of metallic iron that have been brought to Europe, are in the cellular state already described, owing apparently to the total, or almost total, destruction of the transparent substance. But, besides the fragility of this substance, the specimen in question helps very

much to explain the above circumstance, inasmuch as many of the nodules of the transparent substance belonging to it, are in a state of real decomposition. In that state, they are changed into a white opaque substance, which, upon being lightly pressed or squeezed between the fingers, crumbles into a gritty dry powder. This decomposition may be observed to have taken place in various degrees: in many of the nodules, the substance is merely become friable, without being much altered in its appearance; whereas, some of those which are in a state of complete decomposition, are of an ochreous reddish yellow colour; it is, however, easy to distinguish that this colour does not belong to them, but is owing only to the oxidizement of the adjacent particles of iron.

From the above observations, it will not be difficult to conceive the possibility of the total, or nearly total, destruction of the transparent substance; and also, the appearance the pieces of iron must naturally present, when deprived of it. I cannot help observing likewise, that there appears to exist a very interesting analogy, between these transparent nodules and the globules I described as making part of the stones said to have fallen on the earth. This analogy, though not a very strong one, may lead us to suppose that the two substances are similar in their nature, but that the globules are less pure, and contain a greater quantity of iron.

The native iron from Bohemia is a compact mass, similar to the compact part of the large specimen of iron from Siberia, which has just been described: like that, also, it contains a number of globular bodies or nodules; but they are not in such great proportion as in the Siberian iron. They are besides perfectly opaque, and very much resemble the most compact of

the globules belonging to the stones said to have fallen on the earth.

EXAMINATION OF THE IRON FROM SOUTH AMERICA.

I have already observed, that my experiments coincided with those of Mr. PROUST. He obtained 50 grains of sulphate of nickel, from 100 of this mass. The process I have so frequently mentioned, yielded me 80 grains of oxide of iron from 62 of the metal; which indicates about $7\frac{1}{2}$ of nickel, or about 10 per cent.

EXAMINATION OF THE SIBERIAN IRON.

100 grains of this iron, gave 127 of oxide of iron: hence, it should contain about 17 per cent. of nickel.

The yellow substance belonging to this iron, was analyzed in the same way as the globular bodies, and the earthy parts, of the stone from Benares.

The proportions, resulting from the analysis of 50 grains, and from some previous experiments on other particles, were,

Silica	27
Magnesia	$13\frac{1}{2}$
Oxide of iron	$8\frac{1}{2}$
Oxide of nickel	$\frac{1}{2}$
	$49\frac{1}{2}$.

EXAMINATION OF THE BOHEMIAN IRON.

$26\frac{1}{2}$ grains of this metal, left about $1\frac{1}{2}$ grain of earthy matter, insoluble in nitric acid; and, by ammonia, afforded 30 grains of oxide of iron, inducing an estimation of nearly 5 of nickel.

EXAMINATION OF IRON FROM SENEGAL, BROUGHT BY GENERAL O'HARA, AND GIVEN TO ME BY MR. HATCHETT.

In this experiment, 199 grains of oxide were produced from 145 grains of metal: hence, there may be an estimation of 8 grains in 145, or between 5 and 6 per cent. of nickel.

It will appear, from a collected view of the preceding pages and authorities, that a number of stones asserted to have fallen under similar circumstances, have precisely the same characters. The stones from Benares, the stone from Yorkshire, that from Sienna, and a fragment of one from Bohemia, have a relation to each other not to be questioned.

1st. They have all pyrites of a peculiar character.

2dly. They have all a coating of black oxide of iron.

3dly. They all contain an alloy of iron and nickel. And,

4thly. The earths which serve to them as a sort of connecting medium, correspond in their nature, and nearly in their proportions.

Moreover, in the stones from Benares, pyrites and globular bodies are exceedingly distinct. In the others they are more or less definite; and that from Sienna had one of its globules transparent. Meteors, or lightning, attended the descent of the stones at Benares, and at Sienna. Such coincidence of circumstances, and the unquestionable authorities I have adduced, must, I imagine, remove all doubt as to the descent of these stony substances; for, to disbelieve on the mere ground of incomprehensibility, would be to dispute most of the works of nature.

Respecting the kinds of iron called native, they all contain nickel. The mass in South America is hollow, has concavities,

and appears to have been in a soft or welding state, because it has received various impressions.

The Siberian iron has globular concavities, in part filled with a transparent substance, which, the proportional quantity of oxide of iron excepted, has nearly the composition of the globules in the stone from Benares.

The iron from Bohemia adheres to earthy matter studded with globular bodies.

The Senegal iron had been completely mutilated before it came under my examination.

From these facts, I shall draw no conclusion, but submit the following queries.

1st. Have not all fallen stones, and what are called native irons, the same origin?

2dly. Are all, or any, the produce or the bodies of meteors?

And, lastly, Might not the stone from Yorkshire have formed a meteor in regions too elevated to be discovered?

Specimens of the Benares and Yorkshire stones have been deposited, by the President, in the British Museum.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

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ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1801.

1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
				o	o	Inches.		Inches.			
Jan. 1	28	8	0	39	50	29,88	85		S	2	Cloudy.
	50	2	0	47	53	29,70	90		S	2	Rain.
2	41	8	0	41	51	30,00	81	0,350	SSW	1	Fair.
	49	2	0	48	53	30,04	77		SW	1	Cloudy.
3	44	8	0	48	52	29,80	78		SSW	2	Cloudy.
	52	2	0	52	54	29,78	83		W	2	Cloudy.
4	39	8	0	41	52	29,70	78	0,030	SSW	1	Fine.
	45	2	0	45	55	29,76	72		SW	1	Fine.
5	43	8	0	52	53	29,45	78	0,042	S	2	Cloudy. [Much wind last night.
	54	2	0	49	56	29,58	75		WSW	2	Cloudy.
6	38	8	0	38	53	29,90	77	0,080	SSW	1	Fine.
	44	2	0	44	56	30,04	70		SSW	1	Fine.
7	38	8	0	38	53	30,20	78		SSW	1	Cloudy.
	45	2	0	45	56	30,20	77		E	1	Cloudy.
8	38	8	0	39	53	30,14	81		E	1	Cloudy.
	41	2	0	41	56	30,14	83		E	1	Cloudy.
9	39	8	0	41	53	30,14	84		E	1	Cloudy.
	44	2	0	44	56	30,17	82		E	1	Cloudy.
10	40	8	0	42	54	30,18	84		E	1	Foggy.
	47	2	0	47	56	30,18	84		SE	1	Cloudy.
11	41	8	0	41	54	30,21	82		E	1	Cloudy.
	42	2	0	42	56	30,11	81		ESE	1	Cloudy.
12	36	8	0	36	53	30,16	80		ESE	1	Cloudy.
	39	2	0	39	54	30,09	80		E	1	Cloudy.
13	38	8	0	40	53	30,01	84		E	1	Cloudy.
	44	2	0	44	55	29,95	82		SE	1	Cloudy.
14	42	8	0	45	53	29,89	86	0,022	S	1	Rain.
	46	2	0	45	56	29,93	75		NW	1	Fair.
15	32	8	0	32	53	29,93	82				Foggy.
	43	2	0	43	56	29,86	73		SSE	2	Fine.
16	38	8	0	42	54	29,68	83	0,225	ESE	1	Rain.
	46	2	0	46	57	29,68	82		S	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
				°	°	Inches.		Inches.			
Jan. 17	41	8	0	47	56	29,56	87	0,100	S	2	Rain.
	49	2	0	49	58	29,59	79		SW	1	Cloudy.
18	35	8	0	35	55	29,79	77		SSW	1	Fine.
	46	2	0	46	58	29,76	75		S	1	Fair.
19	38	8	0	39	54	29,77	85	0,352	N	1	Cloudy.
	44	2	0	43	57	29,93	74		N	1	Fair.
20	36	8	0	48	54	29,87	87		S	2	Cloudy.
	54	2	0	54	56	29,80	81		WSW	2	Cloudy.
21	39	8	0	41	54	29,99	79		SW	1	Cloudy.
	47	2	0	46	56	29,94	73		NW	1	Cloudy.
22	38	8	0	38	53	29,73	77		SW	1	Cloudy.
	43	2	0	43	55	29,56	70		WNW	1	Cloudy.
23	32	8	0	34	53	29,30	73		NW	1	Snow.
	38	2	0	38	54	29,38	72		N	2	Cloudy.
24	29	8	0	30	51	29,59	78	0,025	NNE	2	Fair.
	36	2	0	36	53	29,71	69		NNE	2	Cloudy.
25	28	8	0	28	50	29,77	70		NNE	1	Fine.
	33	2	0	33	52	29,78	66		NE	1	Fine.
26	24	8	0	25	49	29,78	73		S	1	Cloudy.
	36	2	0	36	52	29,66	79		SSW	1	Cloudy.
27	36	8	0	38	50	29,69	84		SW	1	Cloudy.
	44	2	0	41	52	29,76	80		NNE	1	Cloudy.
28	36	8	0	42	50	29,63	81		SW	1	Cloudy.
	48	2	0	48	52	29,67	77		WSW	1	Cloudy.
29	41	8	0	42	51	29,62	78		WSW	2	Cloudy.
	52	2	0	52	54	29,58	67		W	2	Cloudy.
30	39	8	0	39	52	29,94	76		WSW	1	Fair.
	44	2	0	44	54	29,92	74		SW	1	Fair.
31	44	8	0	45	53	29,78	75		SW	1	Cloudy.
	49	2	0	49	55	29,83	71		W	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Feb. 1	44	7	0	44	54	29,82	76		SSW	1	Cloudy.
	49	2	0	49	55	29,81	74		SSW	1	Cloudy.
2	45	7	0	45	54	29,88	77		S	2	Cloudy.
	50	2	0	49	56	29,90	75		S	2	Cloudy.
3	48	7	0	48	54	29,88	84		S	2	Cloudy.
	54	2	0	54	57	29,88	83		S	2	Cloudy.
4	50	7	0	50	56	29,94	75		S	2	Cloudy.
	57	2	0	56	59	29,92	66		S	2	Fair.
5	48	7	0	49	57	29,98	82	0,054	SSW	1	Cloudy.
	55	2	0	54	60	30,02	72		SSW	1	Cloudy.
6	48	7	0	48	58	29,85	77	0,170	S	2	Cloudy.
	53	2	0	53	60	29,85	70		W	2	Cloudy.
7	39	7	0	39	58	30,20	76		NNE	1	Fair.
	44	2	0	44	59	30,24	70		NE	2	Fair.
8	35	7	0	35	56	30,26	75		E	1	Cloudy.
	43	2	0	43	57	30,23	71		S	1	Cloudy.
9	41	7	0	41	56	30,16	77		E	1	Cloudy.
	40	2	0	40	56	30,16	67		E	1	Cloudy.
10	33	7	0	33	54	30,16	69		NE	1	Cloudy.
	37	2	0	37	56	30,11	72		NE	2	Cloudy.
11	33	7	0	34	53	29,90	75		NE	2	Cloudy.
	35	2	0	34	54	29,86	72		NE	2	Snow.
12	25	7	0	27	50	29,78	70		NE	2	Cloudy.
	33	2	0	33	51	29,64	67		NE	2	Cloudy.
13	27	7	0	27	49	29,61	74		NE	2	Snow.
	30	2	0	30	51	29,66	74		NE	2	Snow.
14	27	7	0	28	48	29,72	74		NE	2	Cloudy.
	31	2	0	31	51	29,68	70		NE	2	Cloudy.
15	28	7	0	31	47	29,52	73		NE	2	Cloudy.
	35	2	0	34	50	29,51	73		NE	2	Cloudy.
16	32	7	0	33	47	29,56	73		NE	1	Cloudy.
	35	2	0	35	50	29,58	70		NE	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Feb. 17	33	7	0	34	47	29,60	73		ENE	1	Cloudy.
	41	2	0	41	50	29,66	69		ENE	1	Cloudy.
18	30	7	0	30	47	29,81	78		NNE	1	Fine.
	39	2	0	38	51	29,84	69		NNE	1	Fair.
19	32	7	0	33	48	29,74	73		S	1	Cloudy.
	39	2	0	39	50	29,50	73		SSE	2	Cloudy.
20	34	7	0	38	48	29,37	73	0,025	W	2	Fair.
	45	2	0	45	52	29,59	63		W	2	Fair.
21	40	7	0	42	49	29,46	82	0,115	ESE	2	Rain.
	50	2	0	50	53	29,31	85		SSE	1	Rain.
22	39	7	0	39	50	29,20	81	0,033	SSE	1	Cloudy.
	44	2	0	43	53	29,16	78		S	1	Cloudy.
23	36	7	0	36	51	29,27	79	0,092	SSE	1	Fair.
	46	2	0	46	54	29,27	68		SW	1	Fine.
24	31	7	0	33	51	29,45	78		W	1	Cloudy.
	44	2	0	44	54	29,62	67		W	1	Fair.
25	38	7	0	43	52	29,68	81	0,022	S	2	Rain.
	51	2	0	51	54	29,55	84		S	2	Cloudy.
26	42	7	0	42	53	29,79	75	0,033	WSW	1	Fair.
	51	2	0	51	56	29,94	62		W	1	Fair.
27	36	7	0	36	53	30,08	76		W	1	Fair.
	49	2	0	49	56	30,01	68		SSW	2	Cloudy.
28	38	7	0	41	54	29,93	79		SSW	1	Cloudy.
	50	2	0	50	56	29,90	72		S	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
				°	°	Inches.		Inches.			
Mar. 1	44	7	0	49	55	29,82	83	0,135	SSW	2	Cloudy.
	55	2	0	55	57	29,89	79		SSW	2	Cloudy.
2	50	7	0	50	56	30,11	84		S	2	Cloudy.
	58	2	0	58	58	30,22	78		S	1	Cloudy.
3	50	7	0	50	57	30,37	84		SW	1	Cloudy.
	59	2	0	59	60	30,38	71		SW	1	Fair.
4	50	7	0	50	58	30,34	81		WSW	1	Cloudy.
	53	2	0	53	59	30,43	63		NW	1	Cloudy.
5	38	7	0	41	57	30,42	74		SW	1	Cloudy.
	51	2	0	51	59	30,36	63		W	1	Fair.
6	45	7	0	45	58	30,24	74		SW	1	Cloudy.
	49	2	0	49	60	30,15	72		E	1	Cloudy.
7	37	7	0	40	57	30,34	69	0,062	E	1	Cloudy.
	44	2	0	44	61	30,40	65		E	1	Fair.
8	31	7	0	31	56	30,28	75		SW	1	Fair.
	43	2	0	43	58	30,29	72		NW	1	Fair.
9	33	7	0	33	55	29,96	78		WNW	1	Cloudy.
	50	2	0	50	58	29,91	73		WNW	1	Fair.
10	42	7	0	42	56	30,00	80	0,055	E	1	Rain.
	45	2	0	45	57	30,01	80		E	1	Cloudy.
11	43	7	0	44	56	29,72	80	0,056	ESE	1	Cloudy.
	50	2	0	50	58	29,56	80		ESE	2	Cloudy.
12	46	7	0	47	56	29,32	82	0,067	SW	1	Cloudy.
	55	2	0	53	58	29,48	68		NW	1	Cloudy.
13	40	7	0	41	57	29,58	77		WSW	1	Fair.
	52	2	0	52	59	29,63	63		W	1	Fair.
14	45	7	0	49	57	29,38	80		SSW	2	Cloudy.
	54	2	0	52	58	29,22	78		S	2	Cloudy.
15	37	7	0	37	56	29,38	76	0,128	NW	2	Cloudy.
	44	2	0	44	57	29,64	67		NW	2	Cloudy.
16	31	7	0	32	54	29,80	73		SW	1	Fine.
	49	2	0	49	58	29,80	68		SW	2	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
				o	o	Inches.		Inches.			
Mar. 17	43	7	o	44	55	29,72	79	0,084	SW	2	Fine.
	53	2	o	51	58	29,57	73		SSW	2	Cloudy.
18	35	7	o	36	55	29,50	77	0,218	WSW	1	Cloudy.
	51	2	o	47	57	29,50	68		WNW	2	Cloudy.
19	35	7	o	36	54	29,71	73		NW	1	Fine.
	48	2	o	47	56	29,83	63		NW	2	Fair.
20	40	7	o	42	54	29,74	76		SW	1	Cloudy.
	48	2	o	45	55	29,29	76		S	2	Rain.
21	36	7	o	38	52	29,18	73	0,160	W	2	Fair.
	47	2	o	45	56	29,14	75		SW	2	Fair.
22	37	7	o	39	52	29,12	70	0,058	W	2	Fair.
	48	2	o	47	56	29,22	62		W	2	Fair.
23	37	7	o	37	52	29,15	75		WSW	1	Fine.
	49	2	o	47	55	29,30	69		WNW	2	Cloudy.
24	34	7	o	36	52	29,78	75		SW	1	Fair.
	50	2	o	50	54	29,77	62		S	2	Cloudy.
25	34	7	o	35	53	30,04	77	0,085	W	1	Cloudy.
	50	2	o	50	56	30,14	63		SW	1	Fair.
26	4	7	o	45	54	30,18	75		SW	1	Cloudy.
	55	2	o	55	56	30,18	70		SSW	1	Cloudy.
27	48	7	o	48	55	30,05	81		SSW	1	Cloudy.
	56	2	o	56	57	30,01	73		W	1	Cloudy.
28	45	7	o	47	56	30,01	76		SW	1	Cloudy.
	59	2	o	58	58	30,03	65		SSW	1	Cloudy.
29	48	7	o	50	56	30,03	73		SSW	1	Fair.
	59	2	o	59	57	30,03	66		SW	1	Cloudy.
30	50	7	o	51	57	30,21	81		N	1	Cloudy.
	54	2	o	51	59	30,32	79		E	1	Cloudy.
31	40	7	o	43	56	30,40	78		NE	1	Cloudy.
	54	2	o	54	60	30,33	66		ENE	1	Fine.

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1801	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
April 1	38	7 0	40	57	30,32	77		NE	1	Fine.
	57	2 0	57	60	30,31	65		ENE	1	Fine.
2	38	7 0	43	58	30,27	73		NE	1	Fine.
	62	2 0	61	62	30,25	60		E	1	Fine.
3	42	7 0	44	58	30,14	72		NE	1	Hazy.
	65	2 0	64	61	30,04	61		E	1	Fine.
4	45	7 0	47	60	29,86	70		W	1	Hazy.
	65	2 0	65	62	29,87	64		NW	1	Fine.
5	41	7 0	41	60	29,80	64		N	2	Cloudy.
	46	2 0	46	60	29,90	53		NNE	2	Fair.
6	33	7 0	36	57	30,07	70		W	1	Hazy.
	51	2 0	50	58	30,01	57		W	1	Fair.
7	40	7 0	44	56	29,53	66		SSW	2	Cloudy.
	48	2 0	47	57	29,38	69		S	2	Rain.
8	38	7 0	41	55	29,40	76	0,248	W	1	Cloudy.
	49	2 0	49	56	29,54	65		NW	2	Cloudy.
9	32	7 0	34	54	29,78	72		W	1	Fair.
	52	2 0	52	55	29,73	57		SW	2	Fair.
10	42	7 0	43	55	29,48	70	0,038	WNW	2	Fair.
	53	2 0	51	56	29,54	65		NW	2	Fair.
11	34	7 0	37	54	29,63	78	0,052	WNW	2	Fine.
	49	2 0	48	56	29,66	63		WNW	2	Fair.
12	32	7 0	32	54	29,91	78		N	2	Snow.
	39	2 0	39	55	29,97	59		N	2	Fair.
13	30	7 0	33	52	30,28	70		NE	1	Fine.
	45	2 0	44	56	30,28	70		NE	2	Fair.
14	36	7 0	40	53	30,25	73	0,022	NNE	1	Cloudy.
	48	2 0	48	55	30,15	77		NNE	1	Cloudy.
15	39	7 0	39	53	30,18	76	0,016	NE	1	Cloudy.
	49	2 0	49	54	30,12	70		ENE	1	Cloudy.
16	39	7 0	42	53	29,86	77		ENE	1	Cloudy.
	51	2 0	48	54	28,87	70		ESE	2	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 17	42	7	0	44	54	29,96	75		NE	1	Cloudy.
	57	2	0	57	57	29,89	64		ESE	1	Cloudy.
18	40	7	0	41	55	29,98	73		S	1	Hazy.
	60	2	0	59	59	30,01	58		WNW	1	Fair.
19	42	7	0	44	57	30,16	70		S	1	Fine.
	63	2	0	62	59	30,13	61		N	1	Fine.
20	45	7	0	49	58	30,16	69		W	1	Hazy.
	65	2	0	65	60	30,11	61		NW	1	Fair.
21	48	7	0	50	60	30,16	71		N	1	Cloudy.
	60	2	0	60	60	30,21	60		NE	1	Cloudy.
22	43	7	0	44	58	30,38	65		ENE	1	Cloudy.
	52	2	0	52	60	30,37	62		NE	1	Fair.
23	42	7	0	45	58	30,37	64		NE	2	Cloudy.
	51	2	0	51	60	30,37	60		E	2	Fine.
24	38	7	0	44	58	30,33	67		E	1	Hazy.
	55	2	0	54	60	30,30	55		E	2	Fine.
25	40	7	0	46	58	30,27	72		E	1	Hazy.
	62	2	0	62	62	30,24	56		E	2	Fine.
26	42	7	0	47	59	30,23	69		E	1	Fine.
	63	2	0	63	61	30,22	55		E	2	Fine.
27	43	7	0	48	59	30,27	62		NE	1	Hazy.
	64	2	0	64	61	30,27	56		E	2	Fine.
28	44	7	0	49	59	30,26	68		NE	1	Hazy.
	61	2	0	61	61	30,23	50		E	2	Fine.
29	44	7	0	48	59	30,19	69		NE	1	Hazy.
	60	2	0	60	61	30,15	57		ENE	2	Fine.
30	43	7	0	48	59	30,10	69		NE	1	Hazy.
	62	2	0	62	61	30,04	55		E	2	Fine.

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1801	Six's Therm. least and greatest Heat.	Time, H. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
			o	o	Inches.		Inches.			
May 1	40	7 0	45	58	30,01	73		NE	1	Hazy.
	54	2 0	54	59	29,94	61		NNE	2	Cloudy.
2	43	7 0	48	58	29,68	73		NE	2	Cloudy.
	53	2 0	52	58	29,65	66		NE	2	Cloudy.
3	39	7 0	44	57	29,72	73		NE	1	Cloudy.
	58	2 0	57	58	29,77	58		WNW	1	Fair.
4	48	7 0	52	57	29,96	67		NW	1	Hazy.
	65	2 0	64	59	29,99	56		N	1	Fair.
5	45	7 0	48	58	30,12	72		NE	1	Hazy.
	64	2 0	62	60	30,14	57		E	1	Fine.
6	43	7 0	46	58	30,16	70		NE	1	Cloudy.
	61	2 0	61	60	30,11	59		NNE	1	Fine.
7	43	7 0	47	58	30,10	71		NNE	2	Cloudy.
	62	2 0	62	59	30,05	60		NNE	2	Cloudy.
8	43	7 0	47	58	30,02	67		N	1	Fine.
	63	2 0	61	60	29,95	57		N	1	Fair.
9	46	7 0	50	60	29,95	65		WNW	1	Fair.
	59	2 0	59	60	29,97	57		WNW	1	Cloudy.
10	42	7 0	47	58	30,11	66		SW	1	Fine.
	63	2 0	63	59	30,11	56		NW	1	Fair.
11	45	7 0	48	58	30,15	68		WSW	1	Fine.
	67	2 0	66	60	30,12	60		WSW	1	Fair.
12	46	7 0	50	59	29,95	70		SW	1	Fine.
	64	2 0	57	59	29,88	72		W	1	Rain.
13	43	7 0	47	59	29,85	70	0,235	N	1	Fair.
	59	2 0	59	59	29,85	58		NW	1	Cloudy.
14	40	7 0	44	58	29,88	73	0,031	SE	1	Cloudy.
	60	2 0	58	58	29,84	60		S	2	Cloudy.
15	49	7 0	50	57	29,64	76	0,133	S	2	Rain.
	60	2 0	60	58	29,58	67		S	2	Cloudy.
16	46	7 0	52	58	29,73	74	0,048	SSW	1	Fine.
	65	2 0	65	59	29,75	62		SSW	2	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
				o	o	Inches.		Inches.			
May 17	51	7	o	54	59	29,68	72		SE	2	Cloudy.
	64	2	o	64	60	29,64	63		SSW	2	Cloudy.
18	51	7	o	52	59	29,73	73	0,195	S	1	Rain.
	61	2	o	60	60	29,75	67		SE	1	Cloudy.
19	51	7	o	53	59	29,87	70	0,050	NNE	1	Cloudy.
	66	2	o	64	60	29,89	60		N	1	Cloudy.
20	46	7	o	50	60	30,01	52		W	1	Hazy.
	66	2	o	66	61	30,00	55		S	1	Fair.
21	48	7	o	50	60	29,92	70		S	1	Fine.
	70	2	o	70	62	29,82	56		S	1	Fine.
22	48	7	o	55	61	29,74	64		S	1	Fine.
	70	2	o	70	62	29,68	59		SE	2	Fair.
23	55	7	o	58	62	29,62	67		SE	1	Hazy.
	70	2	o	68	63	29,62	61		SSE	2	Fair.
24	54	7	o	55	63	29,77	70		S	2	Fine.
	71	2	o	71	64	29,79	54		SSW	2	Fair.
25	50	7	o	56	63	29,78	56		E	1	Cloudy.
	71	2	o	69	64	29,71	54		ESE	2	Fair.
26	52	7	o	57	63	29,62	75		E	1	Cloudy.
	65	2	o	65	63	29,62	71		SE	1	Cloudy.
27	50	7	o	54	62	29,65	73	0,223	SW	1	Cloudy.
	60	2	o	57	62	29,67	70		SSW	1	Rain.
28	50	7	o	54	61	29,59	73	0,185	S	2	Cloudy.
	63	2	o	63	62	29,63	66		S	2	Cloudy.
29	50	7	o	54	61	29,44	72	0,032	SSE	2	Cloudy.
	66	2	o	66	62	29,44	66		SSE	2	Cloudy.
30	54	7	o	57	61	29,54	76		SE	1	Rain.
	64	2	o	64	62	29,47	78		E	1	Rain.
31	54	7	o	56	62	29,53	86	0,377	NE	1	Rain.
	60	2	o	59	62	29,61	82		NE	1	Rain.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
				o	o	Inches.		Inches.			
June 1	52	7	0	54	61	29,76	80	0,048	NE	1	Cloudy.
	60	2	0	59	62	29,85	77		N	1	Cloudy.
2	53	7	0	55	61	29,90	80		E	1	Cloudy.
	62	2	0	62	62	29,90	78		SE	1	Rain.
3	53	7	0	56	61	29,85	78	0,125	SSE	1	Cloudy.
	66	2	0	65	62	29,86	62		E	1	Cloudy.
4	55	7	0	59	62	29,97	77		NE	1	Cloudy.
	68	2	0	68	63	30,00	66		NE	1	Fair.
5	53	7	0	58	63	30,17	74		E	1	Cloudy.
	68	2	0	66	63	30,18	72		NE	1	Cloudy.
6	55	7	0	58	63	30,19	78	0,070	E	1	Cloudy.
	76	2	0	76	66	30,16	56		SE	1	Fine.
7	56	7	0	58	64	30,28	80	0,130	NE	1	Cloudy.
	68	2	0	67	65	30,21	70		NE	1	Hazy.
8	59	7	0	61	64	30,33	69		SW	1	Cloudy.
	77	2	0	76	67	30,29	63		NW	1	Fair.
9	63	7	0	67	66	30,30	72		NW	1	Fine.
	80	2	0	79	68	30,30	62		NE	1	Fine.
10	62	7	0	66	68	30,28	69		N	1	Fair.
	80	2	0	80	69	30,12	58		W	2	Fine.
11	57	7	0	59	69	30,03	62		NW	2	Fine.
	66	2	0	66	69	30,08	53		N	2	Fine.
12	50	7	0	56	67	30,13	62		NW	1	Fair.
	66	2	0	66	67	29,98	55		NW	2	Fair.
13	44	7	0	51	65	29,81	65	0,058	N	2	Fair.
	56	2	0	48	64	29,80	64		N	2	Cloudy.
14	43	7	0	50	60	30,01	66		NE	2	Fine.
	63	2	0	62	62	30,05	55		N	1	Fair.
15	47	7	0	54	61	30,11	70		W	1	Cloudy.
	67	2	0	65	62	30,08	62		W	1	Cloudy.
16	51	7	0.	54	62	30,02	63		W	2	Cloudy.
	64	2	0	64	62	30,04	58		NW	2	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
				°	°	Inches.		Inches.			
June 17	47	7	0	52	61	30,08	64		N	1	Fair.
	65	2	0	64	62	30,08	55		N	1	Fair.
18	48	7	0	53	61	30,08	64		NE	1	Hazy.
	70	2	0	70	63	30,05	54		NE	1	Fine.
19	53	7	0	56	62	30,08	68		E	1	Cloudy.
	70	2	0	70	63	30,08	58		E	1	Hazy.
20	54	7	0	57	62	30,08	64		W	1	Fair.
	73	2	0	73	64	30,04	53		NE	1	Hazy.
21	51	7	0	56	63	30,04	68		E	1	Fine.
	66	2	0	66	63	30,02	61		ENE	1	Fair.
22	46	7	0	54	62	29,96	65		NE	1	Cloudy.
	63	2	0	63	63	29,92	58		E	2	Fine.
23	45	7	0	52	62	29,89	65		NE	1	Cloudy.
	59	2	0	59	62	29,89	64		NE	2	Cloudy.
24	52	7	0	54	61	29,88	67		E	2	Cloudy.
	65	2	0	65	62	29,86	62		E	2	Fair.
25	53	7	0	56	62	29,82	67		ENE	1	Hazy.
	70	2	0	69	63	29,82	60		E	1	Fine.
26	55	7	0	59	63	29,95	65		NE	1	Cloudy.
	73	2	0	73	63	30,03	57		NW	1	Cloudy.
27	57	7	0	59	63	30,20	67		NE	1	Hazy.
	78	2	0	78	65	30,18	55		SW	1	Hazy.
28	54	7	0	57	64	30,26	67		W	1	Fine.
	78	2	0	78	67	30,24	54		W	1	Fine.
29	55	7	0	58	66	30,24	68		SSW	1	Fine.
	80	2	0	80	67	30,14	54		SSW	1	Fine.
30	61	7	0	62	67	29,91	64		SSE	1	Cloudy.
	70	2	0	70	67	29,81	63		E	1	Rain.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
				o	o	Inches.		Inches.			
July 1	54	7	0	57	66	29,69	73	0,960	SW	1	Fair.
	68	2	0	68	67	29,62	60		SSE	2	Fair.
2	54	7	0	57	66	29,60	74	0,056	E	1	Cloudy.
	70	2	0	70	67	29,60	61		SSW	2	Fair.
3	55	7	0	55	66	29,65	92	0,780	N	1	Rain.
	66	2	0	64	67	29,67	65		NE	1	Rain.
4	55	7	0	57	66	29,77	77	0,207	WSW	1	Fair.
	70	2	0	70	66	29,78	61		SSW	1	Fair.
5	57	7	0	58	66	29,71	75	0,108	SSW	2	Fair.
	70	2	0	70	66	29,71	59		SSW	2	Cloudy.
6	56	7	0	58	65	29,67	78	0,042	E	1	Rain.
	69	2	0	69	66	29,63	71		SE	1	Cloudy.
7	57	7	0	60	66	29,72	76	0,026	S	2	Fair.
	73	2	0	73	67	29,71	63		S	2	Fair.
8	59	7	0	59	66	29,60	73		S	2	Fair.
	70	2	0	70	67	29,53	67		S	2	Rain.
9	56	7	0	58	65	29,48	75	0,115	SW	2	Cloudy.
	66	2	0	66	66	29,58	63		W	2	Cloudy.
10	47	7	0	52	64	29,84	68	0,083	NW	1	Fine.
	66	2	0	66	66	29,87	60		NW	1	Cloudy.
11	50	7	0	53	63	29,89	67		SSW	1	Cloudy.
	69	2	0	67	63	29,77	71		S	1	Cloudy.
12	58	7	0	58	64	29,57	83	0,180	SSW	1	Rain.
	68	2	0	66	65	29,56	58		W	2	Fair.
13	53	7	0	56	64	29,82	74	0,035	W	1	Fair.
	70	2	0	70	64	29,76	64		SSW	1	Cloudy.
14	55	7	0	57	64	29,76	80	0,050	SW	1	Cloudy.
	74	2	0	73	65	29,74	61		W	1	Fair.
15	56	7	0	57	64	29,56	77	0,105	S	1	Rain.
	69	2	0	68	64	29,42	68		S	1	Cloudy.
16	51	7	0	53	64	29,39	75	0,240	W	1	Fair.
	64	2	0	60	63	29,38	67		WNW	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
July 17	51	7	0	53	63	29,47	76	0,177	S	1	Cloudy.
	67	2	0	66	63	29,54	63		SSE	1	Cloudy.
18	50	7	0	55	61	29,78	77		N	1	Cloudy.
	72	2	0	69	64	29,84	63		W	1	Fair.
19	53	7	0	55	63	29,95	79		SW	1	Cloudy.
	77	2	0	77	65	29,98	60		W	1	Fine.
20	55	7	0	57	64	30,10	80		NE	1	Cloudy.
	75	2	0	73	67	30,12	62		NE	1	Fine.
21	56	7	0	62	66	30,12	67		SW	1	Hazy.
	79	2	0	79	69	30,10	57		N	1	Fine.
22	61	7	0	66	68	30,10	65		N	1	Hazy.
	79	2	0	79	70	30,12	63		N	1	Hazy.
23	63	7	0	63	69	30,14	73	0,052	NE	1	Cloudy.
	72	2	0	72	69	30,12	66		E	1	Cloudy.
24	54	7	0	58	68	30,02	72		NE	1	Fine.
	69	2	0	69	68	29,95	60		ENE	2	Fair.
25	54	7	0	57	67	29,90	72	0,135	NE	1	Cloudy.
	71	2	0	71	68	29,88	62		NE	1	Cloudy.
26	55	7	0	56	67	29,88	70		W	1	Hazy.
	76	2	0	76	68	29,89	57		SW	1	Fine.
27	57	7	0	60	67	29,93	76		E	1	Cloudy.
	72	2	0	72	68	29,91	66		E	1	Fair.
28	57	7	0	62	67	29,91	75		E	1	Cloudy.
	70	2	0	68	68	29,91	63		E	1	Cloudy.
29	55	7	0	58	67	29,91	78	0,048	ENE	1	Cloudy.
	66	2	0	66	67	29,87	71		E	1	Cloudy.
30	56	7	0	61	66	29,59	94	0,145	E	1	Rain.
	76	2	0	74	68	29,56	68		S	1	Fair.
31	58	7	0	60	67	29,59	77		SE	1	Fair.
	74	2	0	72	68	29,57	67		SE	2	Fair.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
				o	o	Inches.		Inches.			
Aug. 1	60	7	o	62	67	29,67	75	0,030	S	2	Cloudy.
	75	2	o	74	68	29,69	63		SW	2	Cloudy.
2	59	7	o	60	67	29,90	75	0,022	SW	2	Fair.
	78	2	o	74	68	29,93	68		SW	1	Rain.
3	59	7	o	60	67	30,01	81	0,153	SW	1	Cloudy.
	69	2	o	63	68	30,05	87		NE	1	Cloudy.
4	52	7	o	55	67	30,12	85	0,607	NE	1	Cloudy.
	74	2	o	71	68	30,10	62		N	1	Fine.
5	58	7	o	61	67	30,09	75		WNW	1	Cloudy.
	70	2	o	68	67	30,09	68		NW	1	Cloudy.
6	58	7	o	61	67	30,22	70		NE	1	Cloudy.
	74	2	o	72	68	30,22	61		NE	1	Cloudy.
7	59	7	o	61	67	30,34	74		E	1	Cloudy.
	76	2	o	76	68	30,34	56		E	1	Fair.
8	54	7	o	60	67	30,34	71		E	1	Fair.
	77	2	o	73	69	30,26	61		ENE	1	Fair.
9	58	7	o	60	68	30,22	68		NE	1	Cloudy.
	68	2	o	68	68	30,18	63		NNE	1	Cloudy.
10	56	7	o	60	67	30,14	76		NE	1	Cloudy.
	76	2	o	74	69	30,11	58		E	1	Fine.
11	57	7	o	61	68	30,07	75		NE	1	Fine.
	76	2	o	76	70	30,03	56		NE	1	Fine.
12	57	7	o	63	68	30,00	82		NE	1	Cloudy.
	76	2	o	76	71	29,94	61		NE	2	Fine.
13	61	7	o	61	69	29,83	86	0,207	NE	1	Rain.
	71	2	o	70	69	29,78	79		NW	1	Cloudy.
14	61	7	o	61	69	29,85	90	0,510	NE	1	Cloudy.
	71	2	o	71	69	29,89	65		NE	1	Cloudy.
15	61	7	o	61	69	30,07	77		NE	1	Cloudy.
	73	2	o	73	69	30,13	62		NE	1	Cloudy.
16	62	7	o	62	69	30,24	73		E	1	Cloudy.
	71	2	o	70	69	30,27	67		E	1	Cloudy.

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Aug. 17	57	7	0	58	68	30,32	72		E	1	Fair.
	76	2	0	75	70	30,27	62		SE	1	Fine.
18	55	7	0	60	68	30,22	80		E	1	Cloudy.
	74	2	0	73	70	30,18	62		E	1	Fine.
19	56	7	0	58	68	30,17	78		ENE	1	Hazy.
	74	2	0	74	72	30,11	60		E	1	Fine.
20	57	7	0	61	69	30,04	80		E	1	Cloudy.
	79	2	0	79	72	29,97	60		E	1	Fine.
21	59	7	0	61	70	30,04	78		NNE	1	Cloudy.
	71	2	0	71	70	30,05	65		E	1	Cloudy.
22	58	7	0	61	69	30,12	80		NE	1	Fine.
	71	2	0	71	70	30,14	55		E	1	Fine.
23	52	7	0	56	68	30,20	70		NE	1	Fine.
	69	2	0	69	68	30,19	55		E	2	Fine.
24	51	7	0	54	67	30,15	70		SE	1	Fine.
	74	2	0	73	68	30,10	57		NE	1	Fair.
25	55	7	0	58	66	30,17	70		NE	1	Fair.
	72	2	0	71	67	30,16	57		NE	1	Fair.
26	60	7	0	63	66	30,14	65		SW	1	Cloudy.
	71	2	0	69	67	30,10	61		E	1	Cloudy.
27	58	7	0	62	66	30,06	69		ENE	1	Hazy.
	73	2	0	73	67	30,01	55		E	1	Fair.
28	51	7	0	54	65	29,95	69		NNE	1	Fair.
	74	2	0	74	67	29,90	56		N	1	Fair.
29	55	7	0	58	66	29,88	68		W	1	Hazy.
	76	2	0	76	69	29,86	57		W	1	Hazy.
30	60	7	0	61	68	29,85	76		SW	1	Cloudy.
	77	2	0	76	70	29,81	56		WSW	2	Fair.
31	62	7	0	62	68	29,61	78	0,040	SSW	1	Rain.
	69	2	0	68	68	29,57	70		NW	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Sept. 1	50	7	0	53	66	29,78	71	0,057	W	1	Fine.
	69	2	0	68	66	29,83	57		W	1	Fair.
2	56	7	0	58	64	29,72	73		SW	1	Cloudy.
	67	2	0	66	66	29,66	57		SW	2	Fair.
3	54	7	0	55	59	29,72	70	0,045	SW	1	Cloudy.
	69	2	0	69	66	29,71	59		WSW	1	Cloudy.
4	58	7	0	60	62	29,50	81	0,150	SSW	2	Fair.
	67	2	0	66	65	29,40	77		SSW	2	Rain.
5	60	7	0	62	63	29,41	75	0,038	SW	2	Cloudy.
	73	2	0	73	64	29,54	62		WSW	2	Cloudy.
6	60	7	0	60	63	29,44	81	0,325	E	1	Cloudy.
	71	2	0	69	64	29,38	73		E	1	Cloudy.
7	61	7	0	62	62	29,54	78	0,018	WNW	1	Cloudy.
	66	2	0	66	64	29,72	73		NW	2	Cloudy.
8	53	7	0	55	62	30,10	78		NW	1	Cloudy.
	69	2	0	68	64	30,14	64		NNW	1	Cloudy.
9	60	7	0	60	62	30,20	74		E	1	Cloudy.
	70	2	0	70	64	30,20	65		E	1	Fair.
10	51	7	0	52	62	30,18	70		ENE	1	Fair.
	66	2	0	66	64	30,18	60		NE	1	Cloudy.
11	56	7	0	56	62	30,16	67		NE	1	Cloudy.
	66	2	0	65	65	30,11	63		NE	1	Cloudy.
12	57	7	0	57	62	30,00	84		NE	1	Cloudy.
	64	2	0	62	64	29,97	77		NE	1	Cloudy.
13	57	7	0	58	62	29,94	82	0,028	NE	1	Cloudy.
	65	2	0	65	63	29,94	78		NE	1	Cloudy.
14	59	7	0	59	62	30,02	83		SE	1	Cloudy.
	71	2	0	66	64	30,05	74		NW	1	Rain.
15	60	7	0	60	63	30,24	84		NE	1	Cloudy.
	70	2	0	70	64	30,30	65		NE	1	Fair.
16	54	7	0	55	62	30,33	80		ENE	1	Cloudy.
	69	2	0	68	64	30,25	65		ENE	1	Fine.

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				°	°	Inches.		Inches.			
Sep. 17	56	7	0	59	63	30,04	84		NE	1	Cloudy.
	69	2	0	68	65	29,87	73		E	1	Cloudy.
18	62	7	0	62	62	29,60	86		E	1	Cloudy.
	71	2	0	71	65	29,57	71		S	2	Fair.
19	55	7	0	55	63	29,72	77	0,205	WSW	1	Fine.
	69	2	0	69	66	29,83	60		WSW	1	Fair.
20	55	7	0	57	63	29,90	78		SSW	1	Fair.
	67	2	0	67	65	29,89	67		SSW	1	Cloudy.
21	53	7	0	54	61	29,81	73		WNW	1	Cloudy.
	64	2	0	64	63	29,81	65		NW	1	Fair.
22	54	7	0	54	60	29,86	80		N	1	Rain.
	56	2	0	56	61	29,86	76		NW	1	Rain.
23	53	7	0	54	60	29,75	84	0,086	S	1	Cloudy.
	63	2	0	63	61	29,88	75		E	1	Cloudy.
24	48	7	0	48	59	29,98	79	0,197	NE	1	Fair.
	62	2	0	61	60	30,00	69		NE	1	Fair.
25	46	7	0	47	58	30,04	77		SW	1	Fine.
	59	2	0	59	61	30,00	63		NE	1	Fine.
26	51	7	0	51	59	29,90	75		E	1	Fair.
	61	2	0	61	59	29,84	75		E	1	Cloudy.
27	58	7	0	59	60	29,90	92	0,115	SW	1	Cloudy.
	71	2	0	70	63	29,97	68		SW	1	Cloudy.
28	57	7	0	59	62	30,05	87		E	1	Fine.
	67	2	0	67	63	30,04	76		SSE	2	Cloudy.
29	59	7	0	59	63	29,94	87		SSE	2	Fair.
	70	2	0	69	65	29,85	69		S	2	Cloudy.
30	55	7	0	55	63	29,85	81		SW	1	Fair.
	68	2	0	64	64	29,91	73		W	1	Rain.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Oct. 1	59	7	0	59	61	30,16	71	0,046	NW	1	Fine.
	59	2	0	59	63	30,20	63		NW	1	Fine.
2	48	7	0	52	61	30,22	72		W	1	Fair.
	65	2	0	65	63	30,18	63		S	1	Fair.
3	51	7	0	52	61	30,07	77		E	1	Fine.
	65	2	0	65	63	30,05	68		WSW	1	Fair.
4	55	7	0	56	62	29,92	78		S	1	Cloudy.
	61	2	0	61	63	29,81	74		SW	1	Fair.
5	51	7	0	52	61	29,67	78	0,180	SW	1	Rain.
	61	2	0	61	62	29,62	71		SW	1	Fair.
6	47	7	0	47	61	29,66	78		SW	1	Cloudy.
	57	2	0	57	61	29,72	68		NW	1	Cloudy.
7	42	7	0	42	58	29,77	77		SW	1	Fair.
	58	2	0	58	60	29,76	70		SSW	1	Fair.
8	50	7	0	54	59	29,42	74		ESE	2	Cloudy.
	58	2	0	58	60	29,38	77		ESE	2	Rain.
9	49	7	0	50	59	29,55	80	0,130	SSE	1	Fair.
	61	2	0	61	62	29,65	67		SE	1	Fair.
10	49	7	0	50	61	29,68	83				Foggy.
	65	2	0	65	63	29,61	68		E	1	Fair.
11	54	7	0	55	62	29,52	87	0,537	E	1	Cloudy. Much thunder & lightning last night.
	61	2	0	61	63	29,53	76		WNW	1	Cloudy.
12	47	7	0	47	62	29,77	85		WNW	1	Fair.
	58	2	0	58	64	29,84	83		SW	1	Fine.
13	48	7	0	51	62	29,92	82		E	1	Fair.
	63	2	0	63	64	29,92	83		S	1	Fair.
14	55	7	0	59	63	29,98	90	0,020	S	2	Cloudy.
	66	2	0	63	64	29,96	67		S	2	Cloudy.
15	53	7	0	54	63	29,94	86	0,067	S	1	Cloudy.
	60	2	0	60	64	29,80	77		S	2	Rain.
16	47	7	0	47	60	29,71	85	0,090	SW	1	Fair.
	60	2	0	60	63	29,73	70		SSW	1	Fine.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
				o	o	Inches.		Inches.			
Oct. 17	50	7	0	52	62	29,66	80		SW	1	Cloudy.
	60	2	0	58	62	29,51	80		SSE	1	Rain.
18	55	7	0	55	62	29,16	74	0,140	SW	2	Fine. [Much wind last night.
	59	2	0	59	64	29,04	66		SSW	2	Cloudy.
19	46	7	0	47	61	29,48	79	0,065	SW	1	Fair.
	55	2	0	54	62	29,51	69		N	1	Cloudy.
20	39	7	0	39	60	29,69	77	0,014	NW	1	Fine.
	52	2	0	50	62	29,74	63		NW	1	Fine.
21	40	7	0	45	58	29,48	77		WSW	1	Cloudy.
	47	2	0	45	61	29,46	68		NNE	1	Fine.
22	34	7	0	34	57	29,51	72		W	1	Fine.
	47	2	0	47	58	29,57	73		NW	1	Cloudy.
23	45	7	0	46	56	29,97	77		N	1	Cloudy.
	53	2	0	53	59	30,06	69		N	1	Fair.
24	40	7	0	40	57	30,15	76		NNE	1	Fine.
	50	2	0	50	60	30,09	67		NNE	1	Fine.
25	42	7	0	42	57	30,04	82		NNE	1	Fine.
	53	2	0	53	58	30,07	75		N	1	Cloudy.
26	45	7	0	47	56	30,28	79		N	1	Cloudy.
	56	2	0	56	59	30,34	75		NE	1	Cloudy.
27	47	7	0	47	57	30,34	80		W	1	Cloudy.
	56	2	0	56	59	30,25	69		SW	1	Cloudy.
28	46	7	0	47	57	30,08	77	0,185	SW	1	Cloudy.
	49	2	0	49	60	30,16	63		NNE	1	Fine.
29	38	7	0	41	57	30,21	72		SSE	1	Cloudy.
	54	2	0	54	58	30,10	77		S	1	Cloudy.
30	52	7	0	53	58	29,99	82		SW	2	Cloudy.
	59	2	0	59	60	29,92	73		SW	1	Cloudy.
31	55	7	0	56	59	29,98	87		SW	1	Cloudy.
	62	2	0	62	62	29,98	70		WSW	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Nov. 1	56	7	0	56	60	29,70	83	0,060	S	2	Cloudy.
	60	2	0	60	62	29,68	73		SW	2	Fair.
2	59	7	0	59	60	29,36	82	0,395	S	2	Rain.
	56	2	0	56	62	28,85	77		S	2	Rain.
3	37	7	0	37	58	29,88	69	0,381	NW	1	Cloudy.
	42	2	0	42	58	29,80	71		E	1	Cloudy.
4	41	7	0	46	57	29,09	91	1,105	NE	1	Rain.
	50	2	0	48	58	29,12	95		NE	1	Rain.
5	31	7	0	31	53	29,91	67	0,200	NW	1	Fine.
	38	2	0	38	56	30,10	61		NW	1	Fine.
6	30	7	0	30	53	30,33	76		WSW	1	Fine.
	42	2	0	42	55	30,31	72		S	1	Fine.
7	32	7	0	34	52	30,02	75	0,235	NE	1	Cloudy.
	40	2	0	40	54	29,81	73		E	1	Rain.
8	35	7	0	35	52	29,80	84		SW	1	Fine.
	48	2	0	47	54	29,92	80		SSE	1	Fair.
9	39	7	0	39	52	30,07	88		E	1	Cloudy.
	48	2	0	48	52	30,08	87		NE	1	Cloudy.
10	41	7	0	43	53	30,00	88		E	1	Cloudy.
	48	2	0	48	55	29,96	83		E	1	Cloudy.
11	44	7	0	46	53	29,81	92				Foggy.
	53	2	0	53	55	29,75	85		S	1	Cloudy.
12	44	7	0	44	54	29,77	90		WSW	1	Fine.
	49	2	0	48	57	29,84	76		SW	1	Fair.
13	35	7	0	37	55	29,88	82		NE	1	Foggy.
	44	2	0	44	56	29,91	84		NE	1	Rain.
14	43	7	0	43	55	30,05	85	0,073	NW	1	Cloudy.
*	44	2	0	43	56	30,11	82		SW	1	Cloudy.
15	37	7	0	41	55	30,18	85		SSW	1	Cloudy.
	53	2	0	53	57	30,16	86		S	1	Cloudy.
16	41	7	0	47	56	30,18	85		S	1	Cloudy.
	55	2	0	55	57	30,18	78		SW	1	Cloudy.

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1801	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Nov. 17	51	7	0	51	56	30,15	91		SSW	1	Cloudy.
	54	2	0	54	58	30,08	85		SW	1	Cloudy.
18	49	7	0	49	57	29,80	87		S	1	Cloudy.
	51	2	0	50	58	29,70	78		N	1	Cloudy.
19	38	7	0	38	56	29,57	76		W	1	Cloudy.
	42	2	0	42	58	29,61	67		NW	1	Fair.
20	33	7	0	35	56	29,86	78		SW	1	Cloudy.
	44	2	0	41	57	29,81	77		W	1	Cloudy.
21	39	7	0	42	55	29,34	78	0,180	SW	2	Rain.
	44	2	0	44	56	29,41	70		NW	2	Cloudy.
22	33	7	0	34	53	29,47	71		WNW	2	Fine.
	41	2	0	41	56	29,54	66		NW	2	Fine.
23	30	7	0	30	51	29,78	68		NW	1	Fine.
	37	2	0	37	53	29,82	66		W	1	Hazy.
24	36	7	0	43	52	29,55	88		SW	1	Foggy.
	49	2	0	48	54	29,47	90		WNW	1	Cloudy.
25	37	7	0	39	52	29,71	77	0,200	W	1	Cloudy.
	47	2	0	46	54	29,68	72		W	1	Fair.
26	42	7	0	42	53	29,33	73		W	2	Fair.
	45	2	0	43	55	29,40	65		W	2	Fine.
27	34	7	0	36	52	29,18	75		E	1	Cloudy.
	36	2	0	36	53	28,83	89		E	1	Snow.
28	33	7	0	34	50	29,29	85	0,385	NW	2	Fair.
	37	2	0	37	53	29,47	81		NW	1	Fine.
29	26	7	0	26	50	29,61	83		WSW	1	Fine.
	31	2	0	29	50	29,58	84		SW	1	Foggy.
30	28	7	0	34	49	29,05	88	0,080	NE	2	Rain.
	36	2	0	36	51	29,00	85		NE	2	Snow.

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1801	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
				o	o	Inches.		Inches.			
Dec. 1	33	8	0	37	49	28,83	81	0,040	SSW	2	Cloudy.
	39	2	0	38	51	28,92	81		NW	1	Cloudy.
2	31	8	0	33	49	29,18	84	0,045	E	1	Rain.
	43	2	0	43	53	29,21	80		W	1	Fair.
3	32	8	0	33	49	29,58	83		W	1	Fine.
	39	2	0	39	53	29,64	77		WNW	1	Fine.
4	30	8	0	31	48	29,80	82		SW	1	Cloudy.
	40	2	0	40	52	29,64	80		ESE	2	Cloudy.
5	40	8	0	50	51	29,03	94	0,335	WSW	2	Cloudy.
	52	2	0	50	53	28,96	90		S	1	Rain.
6	42	8	0	42	51	28,95	83	0,140	WNW	1	Cloudy.
	46	2	0	45	54	29,17	76		W	1	Fair.
7	32	8	0	33	51	29,49	84		WSW	1	Cloudy.
	39	2	0	39	53	29,53	77		SW	1	Cloudy.
8	33	8	0	35	50	29,03	83		S	1	Cloudy.
	51	2	0	45	53	29,50	82		SE	1	Cloudy.
9	41	8	0	48	53	28,87	91	0,180	S	2	Cloudy.
	50	2	0	50	54	28,65	87		S	2	Rain. [Much wind this forenoon.
10	43	8	0	43	52	29,27	84	0,192	SW	2	Fair.
	48	2	0	47	55	29,40	72		WNW	2	Fine.
11	34	8	0	34	52	29,73	82		SW	1	Fair.
	44	2	0	44	54	29,65	78		SSW	1	Cloudy.
12	31	8	0	31	51	29,62	75		SW	2	Fine.
	38	2	0	38	54	29,57	70		W	2	Fair.
13	31	8	0	31	51	29,67	78		W	1	Fair.
	35	2	0	35	54	29,69	74		W	1	Fine.
14	27	8	0	28	50	29,61	76		NW	1	Fair.
	32	2	0	32	52	29,64	73		NW	1	Fair.
15	25	8	0	26	48	29,70	77		SW	1	Fair.
	35	2	0	35	52	29,57	77		SW	1	Hazy.
16	33	8	0	33	48	29,29	82		W	1	Cloudy.
	31	2	0	31	50	29,29	67		NNW	1	Fair.

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Dec. 17	27	8	0	28	47	29,28	81		W	1	Snow.
	32	2	0	32	50	29,39	80		N	1	Cloudy.
18	24	8	0	25	46	29,66	83		W	1	Cloudy.
	31	2	0	31	49	29,68	81		SW	1	Fair.
19	25	8	0	25	45	30,00	80		NW	1	Cloudy.
	31	2	0	30	49	30,13	77		NW	1	Fair.
20	23	8	0	24	45	30,22	80		WNW	1	Cloudy.
	42	2	0	36	47	30,05	80		S	1	Cloudy.
21	38	8	0	42	46	29,70	94	0,102	WSW	1	Cloudy.
	43	2	0	43	49	29,70	87		SW	1	Fair.
22	35	8	0	36	48	29,85	91		NE	1	Cloudy.
	38	2	0	38	51	29,90	80		NE	1	Hazy.
23	32	8	0	41	48	29,63	88		S	2	Cloudy.
	47	2	0	47	50	29,24	91		S	2	Rain.
24	43	8	0	43	50	29,49	82	0,438	SW	1	Cloudy.
	45	2	0	45	52	29,51	79		SW	1	Cloudy.
25	43	8	0	46	52	29,31	93	0,618	E	1	Rain.
	49	2	0	49	55	29,21	94		SSE	1	Rain.
26	44	8	0	45	52	29,05	85	0,248	S	2	Rain. [Much wind last night.
	48	2	0	48	54	29,04	78		S	2	Cloudy.
27	40	8	0	40	51	29,22	80	0,180	S	2	Fair.
	47	2	0	47	54	29,25	75		S	2	Cloudy.
28	36	8	0	36	51	29,66	83		S	1	Cloudy.
	42	2	0	42	53	29,68	81		S	1	Fair.
29	39	8	0	42	52	29,55	87		S	1	Cloudy.
	43	2	0	42	54	29,60	72		W	1	Cloudy.
30	35	8	0	36	52	29,32	80		W	1	Cloudy.
	39	2	0	39	53	29,25	75		NW	1	Cloudy.
31	30	8	0	31	50	29,76	77		NE	1	Fine.
	34	2	0	33	53	29,87	79		NE	2	Cloudy.

1801.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	54	24	41,1	52	25	42,0	58	49	53,7	30,21	29,30	29,84	90	66	78,3	1,226
February	57	25	40,4	56	27	40,7	60	47	53,1	30,26	29,16	29,77	85	62	73,9	0,544
March	59	31	46,1	59	31	46,4	61	52	56,4	30,43	29,12	29,87	84	62	73,5	1,108
April	65	30	47,7	65	32	48,9	62	52	57,6	30,38	29,38	30,04	78	50	65,9	0,376
May	71	39	57,0	71	44	56,7	64	57	60,0	30,16	29,44	29,82	86	52	66,2	1,509
June	80	43	60,9	80	50	62,3	69	61	63,7	30,20	29,76	30,04	80	53	64,7	0,791
July	79	47	63,0	79	52	63,9	70	61	65,9	30,12	29,38	29,77	94	57	69,6	3,524
August	79	51	65,4	79	54	66,1	72	65	68,2	30,34	29,57	30,06	90	55	68,7	1,569
September	73	46	61,3	73	47	61,3	66	58	62,8	30,30	29,38	29,89	92	57	73,7	1,264
October	66	34	53,2	65	34	53,2	64	56	60,6	30,34	29,04	29,83	90	63	75,1	1,474
November	60	26	42,0	60	26	42,5	62	49	54,8	30,33	28,83	29,71	95	61	79,6	3,294
December	52	23	37,5	50	24	38,1	55	45	50,9	30,22	28,65	29,48	94	70	81,2	2,518
Whole year			51,3			51,8			59,0			29,84			72,5	19,197

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.